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THE

JOURNAL OF SCIENCE.

JUNE, 1885.

I. HOMER COLOUR-BLIND.

By JABEZ HOGG, F.R.M.S., M.R.C.S., &c.

THE faculty of judging of colour with accuracy and precision may doubtless be considerably improved or augmented by education. But whether the colour sense has undergone any appreciable amount of change or development in historic times it is hardly possible to say. The Evolution hypothesis in no way assists in the elucidation of the question; no information whatever is derived from a retrospective examination of two or three thousand years or more, going back to the days of Anaxagoras, or of Homer. Hitherto Homer's biographers have failed to convince scholars that he, like our own Milton, was blind; others, with no better purpose, allege that he simply laboured under a special defect of vision, of a nameless character. More recently it has been said "that, judging by the colour epithets Homer employed in his poems, his organ of light and colour, and by inference that of the Greeks of his day, was only partially developed as compared with that of our own." Mr. Gladstone* first broached this theory, and his conclusions were based partly on the supposed defectiveness of Homer's colour vocabulary, which includes no epithet for either green or blue, and partly on the vague and not unfrequently contradictory manner in which he employed a large number of terms when writing of colour. A later critic†

* The Nineteenth Century, October, 1877.
† *Ibid.*, February, 1885.

has undertaken to prove that Mr. Gladstone's views are altogether erroneous. The question, however, is not one of sentiment; it is assuredly an intricate one, and difficult to decide, since many obstacles stand in the way of a satisfactory interpretation of evidence such as that furnished by the Homeric poems. On a careful consideration of the arguments employed by Mr. Gladstone, and those of his opponent in support of an opposite theory, I am led to think that the Homeric colour defect was due to a totally different cause to that suggested.

In trying to arrive at a satisfactory conclusion on a subject of much obscurity, it is usual to base arguments on what is known with some degree of certainty through the results of science, experiments, and statistics, rather than on doubtful inference from theories not yet determined or positively ascertained. Mr. Gladstone's critic may, however, be unaware of the attention which has been bestowed of late years on a physical defect of vision,—colour-blindness,—and he consequently contents himself by arguing solely from words employed in ancient and foreign languages, the ordinary meanings of which are uncertain, while the subjective impressions which they describe are still mere matter of opinion. He nevertheless advances many interesting facts about the spoken language of India, which, by his showing, presents an identical meagreness of colour epithets with those which characterise the ancient Greeks. But collateral evidence of this kind will not enable us to determine whether Homer's defective colour vocabulary was really "one of language and not of perception."

Those members of the profession to which I belong, and who devote, as I do, much attention to defects of vision, will I believe find no difficulty in accepting the theory that Homer's defect was congenital,—was, in fact, a fault of perception, and not one of poverty of language. If the facts furnished by Mr. Gladstone were the only evidence, they would point to this conclusion,—and to this conclusion above all others,—*viz.*, that Homer was colour-blind. The vagueness of epithets employed by him to denote colour finds a close parallel in the language employed by those congenitally colour-blind. Such persons are not conscious of the defect under which they labour, and this very vagueness of adjectives about colour is at all times one of the common proofs of colour-blindness in testing railway-guards, engine-drivers, and others.

Mr. Gladstone tells us that Homer's colour sense was limited at the "stage at which red and yellow, and possibly

deep purple, are definitely distinguished, but not green or blue. This is no uncommon experience of the red-colour blind.* When a railway-guard confounds together not only reds, greens, and browns, but also, as it occasionally happens, shades of violet and reddish purple, he is pronounced incompletely colour-blind. When, however, he further confounds all shades of colour, having the same intensity of light, his colour-blindness is complete. Judging from language alone, Homer, it would appear, suffered from an *incomplete* form of colour-blindness. In such like cases we are unable to appeal to a common or ordinary objective standard of comparison, or test, for determining the precise value of the colour epithets generally in use. Although with the major part of mankind colour is entirely a matter of subjective impression, the colour vocabulary is, most assuredly, capable of educational improvement. On the other hand, colour-blindness is purely a physical defect of an intractable nature.

Some confusion and a certain amount of annoyance were occasioned, a few years ago, by the published conclusions of one who believed himself to be a connoisseur of pictures, but whose unfitness for the task consisted in his being colour-blind.

Dyschromatopsy, partial colour-blindness, is known to be prevalent among civilised nations. Mr. Gladstone's critic is probably aware of this, although no mention is made of so important a fact, he seemingly preferring to discuss Homer's defective colour system—or, as Mr. Gladstone prefers to put it, "his system in lieu of colour, since it was based upon light and its negative darkness rather than on colour proper"—entirely from a philological point of view, nothing daunted by the formidable character of the obstacles which "stand in the way of a perfectly satisfactory interpretation of the evidence furnished," meaning furnished by Philology alone. Homer, he infers, possessed a certain sense of colour redness, because he frequently employed the word *eruthros;* but as the adjective *red* is frequently used to denote other colours than red, this must be taken simply for what it is worth. To know that "wherever the Hindustani language is spoken *red* is used to designate a great variety of natural objects,—that horses, dogs, cows, tigers, lions, monkeys, in short all animals of a brown or reddish brown colour, are called red, and if of a darker brown (or even

* "White light for the red-blind person is a mixture of the two primary colours in proportions which would appear to the normal eye a greenish blue, verging on black."—HELMHOLTZ.

blue are called black: these are facts of interest to naturalists as well as philologists, but they do not assist us over much in the interpretation placed by scholars on the peculiarities of the Homeric colour vocabulary. It is quite unnecessary to go to India for parallel examples and incongruities of colour epithets, as our own vocabulary and the usage of our own people furnish examples enough. In many agricultural districts brown and reddish brown cows, of all shades of colour, are spoken of as red cows, purple berries as black-berries, and so forth. This form of speech is in no way conclusive of a meagre vocabulary: it is simply a conventional defect of language, a provincialism, which has lingered among the bucolic populations of this country for ages, and it is only to be got rid of by education or by mixing with urban peoples. It is no better proof of a limited or defective colour vocabulary than is the slipshod clipping of words so common among us, and which is excessively bewildering to the intelligent foreigner. An idiomatic phraseology is thought to be in no way significant of any meagreness of vocabulary among civilised nations. With reference, however, to the extent of the colour vocabulary especially, it is a fact that the colour sense can, equally with the other senses and with language as a whole, be cultivated and improved by education.

The aboriginal races of the Earth have heretofore exhibited great ignorance of colour, and their vocabularies are consequently of a very limited nature. But this was not the case with the Greeks of Homer's day, nor of the earlier Eastern races, whose love of colour has been unmistakably made historic in their textile fabrics, mosaics, and frescoes, which have been so wonderfully preserved as a proof of a fully-developed colour sense.

A very large number of our lower class of workmen are quite unable to name colours correctly. Their colour vocabulary, on examination, has been found to be extremely limited. Some trades seem to exert a deteriorating influence: for example, the men working in a gas-factory were examined as to their acquaintance with colours; sixty-five stokers were tested, and twenty-four of them were found colour-blind. Of ordinary working men, a little higher in the social scale, a considerable percentage were discovered to be ignorant of colours. Of 268 soldiers tested only 163 exhibited an intelligent knowledge of colours, while 73 named them indifferently well, and 39·18 per cent were unable to discriminate perfectly between red and green, the majority being, in fact, red-blind. On one of the French railways, where all candidates for employment are very carefully

examined as to their colour vocabulary, of the 1050 applicants between the ages of 18 and 30 examined, 98 were rejected because of their ignorance of colour: a number of these, however, on closer examination, were found to be fairly able to distinguish the difference between red and green,—the two most important signalling colours on railways,—and only a small percentage were finally rejected.

The average percentage of colour-blind, among all classes of men, is 3·5 per cent; of women, only ½ per cent. Among the seafaring class the percentage is also remarkably low. From a Parliamentary Report, issued in 1879, of the examinations of candidates for masters' and mates' certificates in the Merchant Seamen's Service,—and which, oddly enough, are made compulsory on all alike, *except pilots*,—the percentage of colour-blind is given as 0·43 per cent (rather under a ½ per cent). But it appears that, as failures were often reported from ignorance of the names of colours and other causes, those rejected were allowed to present themselves a second time, when a third of the whole number passed the colour tests.

From a later return (1884), moved for by the Member for the University of Oxford, at my instigation, we derive other facts regarding the Mercantile Marine. Of the 85 candidates examined, no less a number than 79 were unable to distinguish a difference between green and red; 38 mistook yellow for red; 45 called blue, green; 16 called yellow, green; 4 black, green; 3 white, green; while others called dark green, yellow,—and blue, red. But, remarkable enough, rather more than a third (29) of the whole number examined, on presenting themselves a second time, obtained certificates from the examiners at other seaport towns, one only succeeding in London.

Colour-blindness, like other defects of vision, affects people in different degrees of intensity, and, like myopia or short-sight, it is frequently hereditary. In several instances I have witnessed its transmission through three generations, and it often becomes more pronounced in after-life, or when the near point of vision begins to recede.

It will be seen, then, that a statistical comparison of defects of perception of colour is always of importance, and especially so when discussing the obscure question of a meagre Homeric colour vocabulary. We might with advantage pursue the inquiry of a defective colour sense to the better educated classes, to men of culture, and who, it will be seen, more often suffer from the evils involved in a partial loss of the colour sense. Among the more highly educated

of all nationalities the average number of colour-blind is 4 per cent, an average in excess of that of all other classes. A man may have a good eye for form and outline, and yet be partially or wholly colour-blind. To select an instance from among many is difficult, but one impresses me more than the rest,—that of Wyatt the sculptor, who at the outset of his career was known as a remarkably good draughtsman. He naturally took to painting, but, as his pictures were observed to present curious incongruities of colour, that involved him in grievous difficulties, he with much reluctance was obliged to abandon the brush for the chisel. He was altogether unable to comprehend the nature of his defect,—indeed refused to believe that he was colour-blind. So of men who have attained to eminence in the world of letters, and whose writings unmistakably betray evidences of a meagre colour vocabulary. A striking example of this occurred in the person of my friend the late-lamented Angus B. Reach, whose life, alas! was prematurely shortened by over-work—brain-disease. He was unable to recognise a difference in colour between the leaf, the flower, and the fruit of plants and trees. His want of perception of colour was wholly unknown to, and unrecognised by, himself, until we sat together at the table of a Paris restaurant. He, wishing to finish his letter to the "Chronicle" newspaper, requested the waiter to bring him some ink. As it often happens, under similar circumstances, the ink was brought in a wine-glass. Reach became absorbed in his subject, while I, seated opposite to him, observed him alternately dipping his pen into his claret-glass and into the ink-glass. I frequently checked him, but presently to my surprise he took up the ink-glass and was about to drink, when I remonstrated, and he then said he could see no difference between the colour of the ink and the wine. On subsequently testing him I discovered that he was completely colour-blind.

From an examination of the Homeric colour sense, as indicated by Homeric colour epithets, and as resting on a scientific basis, and also from a statistical point of view, I venture to suggest that Homer was colour-blind. Indeed an analysis of the Homeric colour vocabulary strongly points to this conclusion. Homer certainly laboured under a physical defect of vision, and this fully explains the limited use of the terms he employed to express his sense of colour, and to which Mr. Gladstone has drawn attention.

II. THE FORM OF THE EARTH.

By Major-General A. W. Drayson, F.R.A.S.

IT is generally allowed by all thinking men that those subjects which have been taught us in our youth, and which have been accepted without investigation, because presented to us under the influence of authority, are very likely to be incorrect.

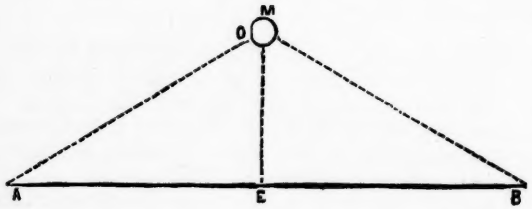
There is a natural tendency in the human mind to follow in the beaten track, and when once any problem has been accepted as correct it is very difficult for us to get rid of the prejudice which we possess in its favour. Consequently error is too often perpetuated by the teaching of the schools, and that which, if a novelty, would be examined in a careful and critical manner, is merely accepted as an article of faith, not of reason. When this article of faith is questioned, or doubted, by a reasoner, he not unusually causes irritation in the mind of the man who has accepted on Faith. Arguments, proofs, or demonstrations, produce little or no effect, and the erroneous teaching is handed down from generation to generation. To be able to *prove* that which we believe is therefore of the greatest importance, and the first demonstrations here given will refer to the form of the Earth.

Even in the present day there are a few persons who assert that the Earth is not spherical in form, but is flat, and they state that the apparent changes in position of the celestial bodies can be satisfactorily accounted for if the Earth is a flat surface.

There is no science which is more rigidly accurate than Geometry. The laws referring to circles, spheres, triangles, &c., are immutable: it is not unusual therefore to find impostors in Science, or those who are merely veneered in Science, sneering at Geometry, because such men would prefer that theories and opinions should take the place of facts. More especially is this the case when such men have gained a reputation for their visionary theories, and fear that an investigation of the rigid laws of Geometry will prove their theories untenable. Men who assert that the Earth is a flat surface will listen to no geometrical proofs. Men who make assertions relative to other supposed forms or movements of the Earth endeavour to ignore Geometry,

which is too exact a science for them. The geometrician, however, *knows* his science is unassailable, and that no opinions or theories can controvert the facts and laws with which he deals.

Individuals who assert that the Earth is a flat surface bring forward the following as a proof that the known facts can be accounted for thus:—They say the moon, for example, may appear 30° above the horizon and to the south of a person in 60° N. lat. It may, at the same instant, appear in the *Z*enith of a person at the Equator; and it may, at the same instant, appear 30° above the horizon and to the north of a person in 60° S. lat. But, say these persons, these facts do not in the least indicate that the Earth is round, because these changes can all be accounted for if the Earth is flat; and they give the following as an example:—



Suppose M the moon, A E B a portion of the Earth and a flat surface; A a locality 60° north, E the Equator, B a locality 60° south.

The Moon as seen from A will have an altitude of E A M, say 30°; at E the Moon will be in the *Z*enith; at B the Moon will have an altitude of E B M, = 30°. Therefore, say these individuals, the fact of the Moon being at different altitudes is no proof of the Earth being spherical in form. Geometry now comes to our aid to prove that a certain result would follow if the Earth were a flat surface, which it is known does not occur. The line M E would represent the sine of the angles M A E, M B E, whilst A M and B M each represents the radius. Consequently, as the angles M A E and M B E are each 30°, the sine M E is equal to half M A or M B. Consequently the Moon seen from A or B would be at double the distance from either of these stations that it is from E. Consequently the Moon would appear to an observer, at E, to have twice the diameter that it would appear to have if seen from B, or A.

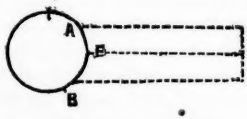
But the Moon, whether seen overhead from the Equator or 30° above the horizon from a locality north or south, appears not half the diameter that it appeared at the Equator, but as nearly as possible the same diameter. Therefore the assumption of the Earth being a flat surface is contradicted by facts.

Again, if the Earth were a flat surface, an observer at A would see a crater at O in the centre of the Moon; whereas an observer at B would not see this crater at all, as it would be on nearly the opposite side of the Moon to that which the observer at B would see. If, then, the Earth were a flat surface, these observers in different parts of the Earth would see a different portion of the Moon; but such is not the case: consequently Geometry proves that the assumption that the Earth is a flat surface is erroneous.

Now what are the conditions that must be fulfilled as regards the Moon when seen from the Earth. They are as follows:—

1. An observer in 60° N. or S. latitude must see the Moon 30° above his horizon, at the same instant that an observer at the Equator sees it at its *Z*enith.
2. The observer north and south of the Equator must find the diameter of the Moon of nearly the same value as it is to an observer at the Equator.
3. The three observers above named must see exactly the same "face" of the Moon.

To account for these three facts we have the following diagram:—



A, an observer north of the Equator, sees the same face of M, the Moon, O as is seen by observers at E or B.

The moon, M, is vertical to an observer at E, and 30° above the horizon to the observers at A and B.

The diameter of M, the Moon, will appear as nearly as possible of the same size to each observer at A, E, and B, because the difference in the Moon's distance from these three points is very slight.

Here, then, we have every known fact connected with the Moon explained by giving to the Earth a spherical form,

whereas if we assume the Earth to be a flat surface we must have certain results occurring, *viz.*, the varied appearance of the Moon's surface and the varied size of the Moon, which we know do not occur.

We need not therefore measure along canals, or apply any such trifling tests, to prove that the Earth is spherical, when Geometry, by aid of the Moon, proves what the form of the Earth must be.

In order to attempt an escape from the preceding geometrical proof, the advocates of the flat surface theory have asserted that the Moon also is flat and shaped like a coin, and therefore only one side can be seen by people in different parts of the Earth. Geometry again proves that this assertion must be incorrect, because if the Moon were in form like a disc it would appear circular in form to a person who was exactly under the Moon, but it would appear elliptical in form to those persons who saw the Moon at any other altitude than exactly overhead.

In these cases we have admirable examples of the power of Geometry to prove or disprove the theories, or assumptions, of visionary schemers. There is a great tendency in some minds to start some theory, and then to cook facts and evidence to make this theory appear correct; and it not unfrequently happens that individuals who thus act imagine they are following out the rules of exact science in what they do. As an example of this peculiarity the form of the Earth may again be taken.

The Earth, it is stated, is not a sphere, but a spheroid. It is flattened at the Poles; consequently the Polar diameter of the Earth is less by several miles than the Equatorial diameter. Now if it be stated that this spheroidal form of the Earth is merely a theory, hundreds of so-called scientific men would probably be annoyed at the ignorance of any person who could make such an assertion, for they would probably state that not only was the Earth proved to be spheroidal, but the actual length of each degree of latitude had been calculated to within one foot. It is quite true that each degree of latitude has been calculated to a foot, but this calculation is based on the theory of the Earth being spheroidal and there being a certain proportion between the Polar and Equatorial diameters.

The actual facts relative to this theory having been so long considered a problem most satisfactorily proved, may now be described.

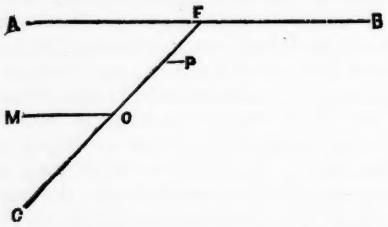
It was, and is, assumed that the Earth in its most ancient condition was in a fluid or molten state. It was again

assumed that whilst it was in this fluid state it began to rotate on its axis. Then, on the suppositions above named, the rotation of the Earth would cause the sphere thus rotating to become a spheroid, and to become flattened at the Poles, and to have a longer diameter at the Equator than at the Poles. An air ball is then made to rotate *rapidly*, and as it does so it assumes a spheroidal form; and this experiment is supposed to be a proof of the Earth being spheroidal in form. A geometrical proof, however, was needed, and this proof was that a degree measured in any meridian near the Equator was shorter than a degree measured near the Poles. In order to obtain evidence of this nature, astronomers and surveyors were employed in various parts of the world to measure arcs of the meridian, and it has been stated these observers proved by their measurements that the length of a degree near the Equator was shorter than a degree near the Pole. And this problem is now considered to have been solved without the slightest possibility of error.

With regard to the last-named evidence it may probably surprise some of our readers when it is stated, first, that the astronomers and surveyors did not prove what it is stated they did prove; and, secondly, that at the time the observations were made the class of instruments used were such that these observers could not prove what it is claimed they did prove.

If we now examine this problem critically, we find that it is first based on two theories. It has been assumed that the Earth was once in a fluid state. The evidence on which this theory is based is very questionable. We must not therefore accept this theory as an established fact. Was the Moon once in a fluid state? if so, when did it begin to solidify? for it is certainly solid now, at least on its surface. If the Earth were once in a fluid state, and heated, and then cooled down, the surface that had cooled down would show evidence of *contraction*, and we should find evidence of this contraction in the "faults" in strata and mineral veins. *All* the faults, however, as is well known to miners, give exactly the opposite evidence. If a fault be found in any vein, the continuation of that vein is never found in the position it would occupy if the Earth's surface had contracted, and the fault had been produced by contraction. Thus if A B be a portion of the Earth's surface, F G a fault, M O a vein of metal, the vein disappearing at O in the fault, the continuation of this vein will be found at P, or at some point between O and F, and is *never* found to be continued

between O and G, as would be the case if the surface of the Earth had contracted. The value of this fact cannot be over-rated. To the reasoning and geometrical mind it is



convincing, but on schemers and visionary theorists it produces no more effect than does the geometrical evidence relative to the Moon on those persons who assert that the Earth is a flat surface.

Then we have another theory based on the first-named theory, *viz.*, that when the Earth was in a fluid state it rotated on its axis; and because an air ball when rotated *rapidly* assumes a spheroidal form, therefore the Earth is spheroidal. Now the Moon is certainly solid on its surface, but rotates once only during about twenty-seven days. It is a mere guess, then, that the Earth rotated rapidly when it was assumed to be in a fluid state. But there is another doubtful question connected with this theory, *viz.*—If an air ball rotate once during twenty-four hours, this rotation would not cause the air ball to assume the form of a spheroid; it is only when it is given some hundred or more rotations in a minute that it does assume such a form. If, now, an air ball were 6 inches, 6 yards, 6 miles, or 600 miles in radius, would a single rotation during twenty-four hours cause this ball to assume a spheroidal form? To spin a ball rapidly, and then to assert that, because this ball assumes a spheroidal form, therefore the Earth is spheroidal, is a false and unsound conclusion, although it is one very readily accepted by the superficial reasoner.

But, it may be stated, accurate measurements have proved that a degree of a meridian near the Equator is shorter than a degree of the same meridian near the Poles. The proceedings connected with these measurements offer an excellent example of that vicious method of cooking facts to suit theories, or the dogmatic opinions of authorities, which cannot be too strongly condemned.

It having become an accepted and popular theory, that the Earth when in a fluid state had rotated on its axis, and as a consequence had assumed a spheroidal form, it was decided that arcs of the meridian should be measured in various parts of the world, in order to prove the accuracy of the theory. Now let us ask what the observers who were sent on these expeditions were expected to accomplish? They were expected to measure a base line, and from this base line to extend a triangulation to the extent of nearly 70 miles. They were then to determine two points north and south of each other, which were exactly 1° apart.

Let us first deal with the surveyors' part of the work, *viz.*, the measurements, and we find that the surveyors had to measure 365,000 feet without an error of 1000 feet, or even 100 feet. This in itself is a work of great difficulty, and one which the surveyors of former times, especially in a rough country, could not fairly accomplish.

Next we have the astronomical problem to consider. The latitude of two points is to be ascertained to within 1″, and with portable instruments. What are the probabilities of this being practically accomplished? With the most perfect instruments, and thousands of observations made each year, the latitude of Greenwich Observatory fifty years ago was 2″ in error; that is, the latitude then assigned to the Observatory differed 2″ from that which is now assigned to it.

A second of latitude may be taken as about 120 feet,—consequently 2″ would be about 240 feet. This error at each station would amount to 480 feet, and this error might occur after a hundred years of observation and with the most perfect instruments. A far greater uncertainty must exist when observations were made during only a few months, or years, and with instruments inferior to those now in use at Greenwich. To claim, therefore, that observers one hundred years ago proved that a degree of the meridian measured near Peru or at the Cape was a certain number of feet shorter than a degree of a meridian measured in England, is one of those pleasing but visionary delusions in which theorists too often indulge.

But a far more amusing delusion exists in connection with certain measurements made in former times to ascertain the tenth millionth part of 90° of latitude. The French being anxious to introduce an exact standard of measure, *viz.*, a tenth millionth part of the arc from the Equator to the Pole, employed several surveyors and astronomers to measure arcs in various parts of the world. These observers, as was

naturally to be expected, differed in their results. The principal rivals were M. Borda and M. Bouguer. M. Borda made the metre 3·0807 of the "Pied de Roy"; but M. Bouguer made the metre 3·0794. The latter gentleman it appears had friends at court, and, lest the learned men of the time should have any doubt on the question, M. Bouguer's measurement was settled by an Act of the Legislature to be accurate (!) and M. Borda's wrong, and thus the French metre was established as the most accurate standard in the world, but not by any scientific proof.

Another experiment was carried out some years ago to obtain, as was supposed, additional evidence that the Earth was spheroidal in form, *viz.*, experiments with the pendulum. The results of these experiments, however, have not been of such a definite character as to be satisfactory, except to those theorists who have already come to the conclusion that the Earth must be spheroidal in form *because* it rotated when in a fluid state.

That the Earth is spheroidal in form is possible, but the evidence claimed as proving this is either mere guess or the arbitrary assumption of authority. The assertion that the Earth was once in a fluid state is a guess, disproved by the facts known to exist in connection with "faults" in mineral veins. That the Earth rotated when in a fluid state is another guess, based on no evidence whatever. That the surveyors and astronomers of the past proved that a degree of latitude near the Equator was shorter than a degree near the Poles is claiming that these surveyors with portable instruments, and during a few months, obtained more accurate results than had been obtained at Greenwich Observatory during a hundred years.

The question as to whether the Earth is spherical or spheroidal is not one of very great importance, but the manner in which this problem has been dealt with in former times supplies us with an example of the unscientific and prejudiced manner in which some theories are considered to be satisfactorily proved.

That the Earth is spheroidal has never been proved. That the Equatorial diameter exceeds the Polar diameter by a certain number of miles, yards, and feet is a theory only not corroborated by actual facts, though certain assumed evidence has been cooked to agree with the theory. Those persons who can believe that the Earth has contracted, in spite of the evidence shown by the faults in mineral veins, occupy a similar position to those who assert that the Earth is a flat surface, in spite of our seeing only one face of the

Moon; they cannot appreciate the force of an impossible, but cling to a theory, and a preconceived opinion, in spite of it. The manner in which geometrical facts are ignored by many men in the present day, and mere visionary theories are put forward as realites, teaches us that we are in great danger of committing the errors of the ancients, who claimed to know the influence produced on the seasons, and on men's destinies, by the various planets, but were totally ignorant of the form of the Earth or that it rotated on its axis. Theories based on facts are probabilities, but when theories are first started, and then facts are arranged and selected to confirm these theories, there is a great danger of delusions being established by "authority," and of error being taught as truth.

III. SOUTH GEORGIA.

By the Count O. Reichenbach.

THE observations there made by the German Expedition have surprised scientists. The island lies—64 miles long by 30 at the widest—in 36° 5′ W. long., between 57° 31′ and 55° 10′ S. lat. The Expedition remained 352 days, from September 15th, 1883, to September 3rd, 1884. Its station at Royal Bay was surrounded by mountains, with huge glaciers, 900 to 1200 feet high, rising farther inland to 6000 and 7000. No month was free from frost; 30 per cent of the hours of observation showed a temperature below freezing-point. The mean temperature for the 353 days, which would have been slightly increased by the 12 wanting to the year, was 1·666° C.; for February, the warmest month, 5·35° C.; for June, the coldest, −3·22° C. The maximum reached in February was 14°; the minimum in July −3·22°.

In 1880 I wrote ("On some Properties of the Earth")—"At the isotherms of 1·666° C., of which that at the South is quite maritime and almost without curving, the equilibrium of temperature between South and North is re-established; the isotherms coincide, each in its mean, in

both hemispheres, with their bases or parallels; they divide the hemispheres in proportion 1 : 4·78," in 55° 48′ latitude. The island with its glaciers produces a bend of only 1° 7′ of latitude towards the Equator in the isotherm of 1·666°.

The Rev. Professor Samuel Haughton, in "Six Lectures on Physical Geography, 1880," gives, on p. 108, the temperature for 55° 48′ S. lat., 4·78° C.; for 54° 31′ S. lat., 5·29° C.; for 36° 5′ W. long., even 1·22° C. higher.

The mean temperature of the globe was generally given, according to Dove, 14·44° C.,—15·55° for the northern, 13·33° for the southern hemisphere. I made the mean for the globe 15·22°, and the temperature for the two hemispheres equal. Of late I met the figure 15·22° for the globe; by whom determined and by what method I did not see.

The range in South Georgia between the coldest and warmest month was only 8·77° C. What I said of the Antarctic meteorological zone, after having shown that its mean temperature is higher than that of the Arctic, applies to this Sub-antarctic region. "It is generally thought that the Antarctic regions are considerably colder than the Arctic. Those navigators who penetrated farthest in the Antarctic regions did not support this view; the animal life of its seas contradicts it. Remoteness, the low temperature of the warm season, which does not prove the great cold of the long nights, and the imposing icebergs and masses of pack-ice, have produced wrong impressions."

IV. ON THE CELLULAR AND EXTRA-CELLULAR TRANSPORTATION OF NUTRIMENT.

IN consequence of prolonged researches on the absorption and assimilation of peptone, Dr. F. Hofmeister arrives at the following conclusions, which he has given to the world in the "Archiv für Experimentelle Physiologie":—

The absorption of peptone in the intestine is no simple mechanical process of filtration or diffusion, but a function of definite living cells, the colourless blood-corpuscles.

These play, in supplying the system with albumen, a part similar to that of the red globules in respiration. The "Naturforscher" summarises the theoretical outcome of the author's results. His conclusions are of the greater importance as showing that not merely the transformed albumen or peptone, as proved by Hofmeister's researches, and the oxygen, according to less recent observations, but, as it has been shown by Wiedersheim and others, the fat in the higher animals and all the nourishment in the lower forms,—even, according to De Vries, the nutritive matter for plants,—are taken up by cells, or by protoplasm, and thus transported to the parts where they are required.

The continued wear and tear to which the organs of the animal body are subjected necessitate a constant supply of fresh material, as such serve the manifold nutritive substances which are conveyed from the digestive system to the blood, and by it to all the organs. According to the general view these food matters on the way to their destination exist in the state of simple solution, or, in the case of fat, as emulsion. The impelling forces which convey them to such cells as are in need of support are simply the circulation of blood and diffusion. That any portion of the nutrient principles can become temporarily or permanently part and parcel of movable cells, in the coats of the intestine or in the blood-vessels, and can thus carry out the function in an organised form, seems, according to this view, simply excluded. This supposition, however, extends merely to the nutriment introduced through the medium of the digestive organs. For the portion of nutriment taken up through another channel, *i. e.*, the oxygen, Science has long ago come to a very different conclusion, as far, at least, as the higher animals are concerned.

The small quantity of oxygen which can exist in the blood in a state of simple solution cannot even come into consideration as a source of this essential. A sufficient supply and a proper distribution are effected only by the circumstance that the blood-discs become charged with oxygen in the lungs and deliver it again at the places where it is required. Blood containing no discs would be quite incapable of transporting oxygen from the lungs to the other organs.

Dr. Hofmeister, for the sake of brevity, terms that mode of conveying nutrition which involves the co-operation of figured elements "cellular transfer." If the supply is effected, in accordance with the common view just expounded,

without the co-operation of cellular elements, this would be "extra-cellular transportation."

That the difference between these two forms of the conveyance of nutriment is not merely theoretical, but involves an essential difference in our conception of the process of nutrition, will appear from the following simple consideration:—Nutrient matter dissolved in blood or lymph, and consequently circulating in an extra-cellular state, must naturally follow the existing currents of liquids, in as far as the laws of diffusion and filtration permit. If they have been taken up by the blood, they are conveyed by it to all the organs in one and the same degree of concentration. Along with the transuding phasma they permeate every tissue, whether it requires them or not, and arrive finally in the organs of secretion. Whether they are held back at these points of exit, or find a passage and are thus lost to the organism, depends only on the arrangement of the secreting apparatus and on its condition for the time being.

Very different must be the behaviour of nutrient matter which enters into the circulation in combination with cells. Here there is no question of their uniform distribution in the liquids of the body. The cells containing nutriment are small, itinerant magazines, in which relatively large proportions of nutrient matter are heaped up in a single point. They are not at all subject to the laws of diffusion, and obey the existing liquid currents only within narrow limits. If they have no inherent motive power the coats of the vessels present to them an impassable limit; but if they have an automatic motion, and are capable of travelling, they are, when outside the current of the blood, to a great degree independent of the air-current. In either case there is the possibility of supplying the organs with nutriment exactly as required. To fulfil this condition we need merely, in the former case, suppose that the circulating cells charged with nutriment give up their cargo to the surrounding tissues through the capillary lining to such an extent as it is required. Tissues which for the time being do not require a supply are simply passed over in the distribution. Such an arrangement, we know, prevails in the supply of oxygen to the tissues.

In the second case, if we admit that the nutrition-carrying cells are capable of travelling, the affair is still simpler. The cells can arrive directly at the place required, and can be utilised either with their entire mass or only with a part of their cargo. This arrangement, even more than the first case, permits the direction of large quantities of nutriment

to single points in particular need of a supply. This arrangement appears realised on a pathologically enlarged scale in the emigration of the colourless blood-corpuscles in cases of inflammation.

The cellular combination of nutritive matter explains very simply the formation of receptacles for reserve-matters in which, during a time of superfluity, the nutriment taken up is stored in an organised form, so as to be at the disposal of the organism in times of need without the necessity of previous important chemical transformations.

"Cellular transportation" is exceedingly important for the conveyance of matters insoluble in the liquid of the blood, and therefore incapable of diffusion. The difficulty of conceiving the entrance of such substances into the tissues appears at once annulled if we assume that it is effected through the mediation of transportable cells. This comes into play for the transfer of medicines and poisons perhaps even more than under normal conditions.

The greatest advantage, however, of "cellular transportation" is that it prevents a waste of the nutritive material which has been secured for the organism. We know that many very important nutrients (sugar, peptone, &c.), if they are introduced directly into the blood, even in small quantities, they are expelled through the secreting organs, whilst on the reception of much larger quantities of the same substances by way of the intestine, under physiological conditions, nothing similar is observed. This different result appears scarcely conceivable if we assume that nutriment is conveyed to the organs in the state of solution; but if we suppose a cellular combination it seems not merely intelligible, but a matter of course.

As regards the manner in which the nutrient matters in cellular combination are distributed at the places where they are needed, Hofmeister points out that already many circumstances are known which indicate that the amœboid movements of the cells are affected by various conditions, and possibly by the absence of those matters of which they contain an excess. Here further experiments and observations are requisite. The "Naturforscher" refers here to the experiments of Pfeiffer in which the direction of the movements of spores was effected by the chemical character of the surrounding medium.

Finally, Hofmeister intimates that the conveyance of nutriment may be both cellular and extra-cellular. Even in cellular transportation processes of difference between cell and cell may not be wanting, and extra-cellular transportation

—as it is admitted in the cases of water and salts—will fall transitorily under the influence of the cells. To what extent, and for which kinds of nutriment these two modes of transportation come into play, can be determined only by experiment.

V. THE CONCEPTION OF IDEAS AND THE PROGRESS OF THOUGHT.

By D. Y. Cliff.

IT is curious to consider what the first thought was like: was it transitory, or a beginning that had no break? The old Semitic idea is that man "awoke" to thought, but of such a nature that it could not distinguish "good and evil:" on attaining that much discernment it saw that man must "surely die:" man recognised himself amongst Nature generally as a passing guest. Though the first origin of thought cannot perhaps be traced, yet it is not impossible to follow historically distinct phases of development (or alteration); and more vaguely to get at something like a ratio of time to progress. Suppose that man's development had not taken a self conscience mental turn; supposing him still to have survived with his inherent tendency of progression, what would have been the result but a spended evolution of idleness? Idleness had evolved a being who was superior to all accidental and surrounding forces,—a naked creature probably living on air, &c.

But what is an unconscience existence but a non-existence so to speak? Nature exists to us, but not to itself. And as only the mind makes us aware of its existence, it is not quite an empty, though strange remark, to say—mind actually creates matter. What is the object of Nature's existence when it is unknown to itself? Our minds are the minds of all matter; and a consequence of it. If matter does exist, it must, one would think, have been prior to thought; but if mind creates matter then both (*e.g.*, to use words) simultaneously appeared: for what is one without the other? To the body-sensuous "the spirit" is non-material, and so to

the mind the body is untangible. However, man has become aware that he *is*, and it is generally thought has allowed his mental proclivities to overreach his physical devolopment, altering, perhaps, even that from its original path. Man has attained to the conception of the immortal. This to my mind lends favour to the theory of prëadamite races. Perhaps once thought and memory passed to the child, *i. e.*, the crude consciousness of an existence. The hazy mind in the long, long life of brute man probably could not pass back to its beginning. A dim idea of the everlasting would impress itself on his progeny: and as he became more "awake" it would be constantly impressed on him by the permanency of Nature, by the unpierceable heights of the silent blue above, and the unbound and unconceivable expanse of the ocean at his feet. Then he tasted of the tree of life and realised that he must die, but he could not conceive that he should drop out of time. But, in spite of what is often said to the contrary, I do not think the idea of immortality is intuitive.

But let us turn to actual facts. We find that the mind varies in different races, but that certain stems are common to all their diversities. It is observable in the expression of the face; in the gait; speech; grammar; expressions and phases of thought; illustrated in art, literature, manners, dress, fancies, religions, and so forth; the most striking being those apparent to the eye, their handcraft, and their social doings. To this we trace the reason why no religion can be universal unless the whole be inhabited by one race.

Nor are peculiarities matters of accidental growth—this is contradicted by their constancy in variation. Thus the oriental does not only differ in choosing to consider his left hand the place of honour, but his book reads doubly backwards, and even his letters slant in an opposite direction. Yet this oppositeness does not extend to physical facts, such as being left-handed for instance, which is a proof of its subsequent development, or mental divergency. The straight line or a circle is the same thing to all men, though they may vary in the direction in which they draw them: we might say it is the fact of *order* impressed on the mind.

This suggests an inquiry: how have our notions of rectilineal lines and perfect circles become impressed upon our minds, when in all Nature it is far from the rule and scarcely perceptible anywhere? It is the exception. The sun is round, and so is the iris of the eye, but where are the vast quantity of horizontal, perpendicular, and parallel lines that haunt the human mind, and result in order—so absent

in Nature generally? Is it merely a matter of survival? He who threw his weapon straightest, he who made for the shortest distance between two points, and so forth? Animals, though, seem to have this consciousness, though perhaps an insect does not. But can any historical improvement in a race be traced? I think so. I am most certainly inclined to the belief that old sculptures, paintings, &c., stiff crude, or whatever else they may be, represent the exact state of the mind of the people amongst whom it was produced, otherwise it would have been detected and corrected—must have been. On these grounds I should say the old Egyptian was "his beard over again;" and the stiffness of the Assyrian designs actually existed in their intercourse and modes of thought: their literature would be the same. This is why the Greeks were alike beautiful (according to our conceptions) in their face, their art, and their writings. Is it not known that the old oriental music was wildly discordant and meagre? But with the Aryan Greek did not her earliest poet know the power of the Syren's music? At last the mind had broken free: unfettered it scudded before the wind of fancy and taste whither so ever it would: thought flowed easy, and so consequently the fold of the garment on the statue and the pen of the poet in his verse. Can we trace the pure descendants of these Assyrians and Egyptians? What is their artistic taste to day? Can a Hindu whose fathers carved the uncouth monsters scattered over the land of India *now* paint like a European or attain any excellency in sculpture according to European standards of judgment—beyond mere imitation? I have not the information: but in case it is so, which I am inclined to doubt, the question of a development in the Hindu mind is debateable. It is possible from religious prejudices or erroneous fashions to adhere to old conceptions of beauty long after common sense scorns it. The differences strike me as mental incongruities rather than faulty or uneducated manipulation.

Then if we turn more particularly to architecture since Christ's time for a visible exhibition of the mind of nations we are impressed with various thoughts. To me they all tell their story. It is said that the Gothic pillars may be continued any length without destroying the beauty of the effect. Is this not a visible exhibition of the latent ideas floating in the mind of the principles of Christianity "looking heavenwards?"

The Gothic architect stands beneath and between pillars that tower away heavenwards; he is essentially at the *bottom*, just in the same way the eye of the Norman architect is in

the *middle* of his arch; and though it may be fanciful, I think the Byzantium horse-shoe arch shows that the architect's eye looked at it from below, *above* him. Yet he was not wholly excluded from the space by reason of the lower opening. Of course they would not have said such were their conceptions, but such appears to me to be their latent origin, and from this we can draw conclusions as to their mental train of idea. May not the bounding arch represent a circular heaven? where was it in the temples of the ancient monarchies? The straight line preceded the circle. Man considered the plain before he looked up to the sky.

From these reflections naturally arises the question, looking at thought as seen from its visible exhibitions, what progress have we made? Are we not at exactly the stage of development as the paltry Republics of the Peloponnesian peninsula and the Hellenian mainland? It is admitted they are not excelled. Are we compelled to acknowledge a sort of "separate creation" for the Aryan, whose mental capacity is constant and equal through all ages, and only varies as a result of accumulative experience? I can't answer this off hand, but reply by another incidence in the world's history, viz., the complete development of the Jewish religion, on which Christ put the headstone, when he said Moses had insisted on an eye for eye, but He bid them offer the other cheek to the smiter. It is needless to repeat the change then in *one race*, but it did occur; and ordinances first issued as the commandments are now moral certainties.

There is one thing noteworthy, I think, in modern everyday writing, that is its crowdedness. The mind would ever seem to be crowding in with fresh ideas, edgeway as it were; a whole string of words will intervene between a proposition and a verb, a sentence three quarters told is interrupted by an explanation, beatitude, or otherwise even longer than the sentence itself, and so forth. Exactly so is the modern mind and modern architecture. I have seen somewhere that Herodotus, the early historian of Greece, ran on in his writings almost entirely without a full stop. Modern expression is always seeking too many at once. I think it was Hamilton who made it out that the human mind was capable of conceiving its distant ideas at one time; but we are improving on that, with a necessary consequence of confusion; if, however, we devote ourselves to one idea, is not the extra power of the modern mind made apparent by our achievements? If we only "kept it up" we must grow wiser.

VI. ON THE RECOGNITION OF NOISES.

ON an examination of the question with what part of the auditory organs we recognise noises, S. Exner, as far back as 1876, came to the conclusion that we perceive them with the cochlea and by means of the same tissues with which we hear sounds, that is by the Corti's fibres resting upon the basilar membrane of the cochlea and containing the terminal expansions of the auditory nerve. He founded this view upon Seebeck's experiment, in which a Savart's wheel from which so many of the teeth had been removed that it no longer gave out a sound, but merely a noise, still occasioned the feeling of an elevation of sound when it was made to revolve with greater velocity. He further caused two electric sparks to strike in succession, and on reducing the interval between them he recognised in the noise an elevation of tone.

Von Helmholtz, in the fourth edition of his "Treatise on the Perception of Sound" (Lehre von den Tonempfindungen), adopts Exner's view. He assumes that a single impact, if sufficiently strong, throws the entire elastic apparatus of the basilar membranes with all its organs into a strong initial movement, which then dies away in every single part in its peculiar period of vibration. Thus all the nerves of the cochlea are excited, and thereby the perception of the noise is effected without the decided prominence of any given sound. If the pressure of the wave of air upon the drum is prolonged the special vibrations of those parts of the basilar membrane which oscillate more rapidly than the duration of the pressure are enfeebled or suppressed, whilst those which vibrate more slowly are intensified. These parts of the basilar membrane give rise to the impression of a certain pitch of sound which is mixed with the noise. Still more distinctly is a certain pitch of sound manifested in a noise when the aërial pressure varies several times in succession from positive to negative. Thus we may have all the intermediate stages between noises without any definite pitch and sounds with a pitch. Professor von Helmholtz considers this a proof of Exner's theory that such noises are perceived with the same parts of the ear which serve for the distinction of sounds.

Setting out from these considerations, Ernest Brücke has recently published in the "Transactions of the Vienna

Academy," and in the "Naturforscher," a treatise on noises. He shows that if noises are heard with the same nerves as sounds of deep or of high pitch there must exist a subjective relation between noises and sounds of great wave lengths, and again respectively between noises and sounds of short wave lengths. This is supported in fact by daily experience. Music represents the discharge of cannon by the sounds of the drum, and the meaning is understood. The intensity of the detonation does not here come into play, since no one can confound a distant cannon shot with a pistol shot close at hand. On the contrary, a cannon shot, a rifle shot, the snapping of a percussion cap, and that of a small electric spark, form a series from a deep to a high pitch, so that noises also can be divided into low and high, according to the regions of the cochlea, which are principally excited in each case.

The tone of the tissues whose nerve-fibres are most strongly excited by a noise, and which determines the pitch of the explosive noise, must appear the more distinctly in its peculiarity the more distant the source of the noise. For whilst a cannon shot near at hand certainly excites all the fibres of the auditory nerves, a similar shot in the distance scarcely affects those nerves which are more feebly affected, so that a part only of the auditory nerves comes into play. Indeed, in the latter case the relation with a deep sound is much greater than in the former. With high explosive noises this difference is not so striking, because they are not loud enough to admit of a sufficient variation in the distance.

The better to understand what happens we must first reply to the question as to the simplicity of explosive noises, that is whether they consist of one wave or of a series of waves. Brücke, therefore, instituted experiments with different noises by means of a Koenig's apparatus for demonstrating the flame-images of the vowels—somewhat modified—and he observed that a good clap with the hands produced as a rule only one projection in the flame. The explosion of soap-bubbles filled with detonating-gas gave also generally only one projection. If a small organ-pipe (1050 vibrations) was sounded simultaneously to measure the length of the sound-waves the ignition of percussion caps gave one projection only, while without the organ-pipe several such were obtained. In the former case the feeble secondary impulses were overpowered by the stronger impulse of the pipe.

Brucke next examined explosive noises resembling the

report of fire-arms, but more feeble and selected for this purpose lead pipes from which a stopper was driven by compressed air with a strong puff. This produced a series of projections of decreasing strength. But if the tubes were filled with loose cotton-wool so that the fluctuation of the air was checked, the stopper flew out almost without noise. If secondary fluctuations of the air occur in the lead tubes they must happen also on discharging a rifle or a cannon, and we can no longer deduce the deep tone in the report of the latter exclusively from the dimensions of the primary wave. We hear in the report of fire-arms a very brief sound, a sound too short to be recognised as such, and whose primary impulse, when sufficiently near, simultaneously excites all the cochlear fibres.

In a child's rattle, the noise of which is the higher the more the tongue is shortened, in the blows of a hammer, in stamping, &c., the noises must be judged in the same manner. The character is determined by the secondary vibrations of the parts which come in contact, and of those which are in such connection that they are simultaneously set in vibration. If they last long enough we have the production of a sound; if they are too short we have a noise the character of which depends on the period of the secondary vibrations.

If we examine experimentally what influence the length of a sound-wave exerts upon the auditory sensation which it produces, soap-bubbles filled with detonating gas are the most suitable. They must not exceed a certain size, otherwise all the fibres of the auditory nerves are in all probability affected together. The experiments showed that with a suitable mixture of air and hydrogen the large bubbles gave a duller, therefore a deeper, sound than the smaller ones, though the explosions showed only a single projection, and consisted therefore only of a single wave. The acoustic difference appears therefore with a single wave just as in the explosive noises which, besides the primary wave, produce also secondary vibrations. In both cases the character of the sound is determined by the especial excitement of a part of our auditory nerve fibres.

It is known that no deep tone can be produced by the movement of masses of air caused by single high explosive noises, even if the separate impulses of the air follow slowly upon each other. On this subject we can refer to former experiments with Savart's wheel. The reason must be that each single one of these noises does not set the lower tissues of the internal ear in such a vibration that its

effect still exists when the second impulse follows, and therefore a repetition of each impulse cannot produce a summation of the excitement of the deeper-toned nerve-tissues. When, therefore, acute sounds follow upon each other in long intervals, we hear them singly as noises, because we hear them with parts of the internal ear which are tuned so high that they die away in a shorter time than the interval between the single noises. When this is not completely the case, or when the perceptions of the single impulses melt into each other, we hear a chirping, or, if the impulse is stronger, a rattling or a creaking.

Brücke corroborates this view by a series of experiments, with Savart's wheel, and with children's rattles: he shows that, for the experiments to be clear, every secondary vibration in any part of the apparatus must be eliminated. If this is not the case we obtain deep tones. When the secondary vibrations are checked as far as possible, a slow repetition of the impulse produces merely noises, but a rapid repetition gives distinct tones. Brücke obtains the same result with telephones, which produced noises if the electric impulses were slow, but tones if they were very rapid.

In his researches Brücke arrives at the conclusion that we have no need to assume the existence of distinct nerves for the perception of noises. According to the consideration advanced by Prof. von Helmholtz concerning the cochlea, we suppose that tones are produced when certain groups of the cochlear fibres undergo prolonged excitement, and that the tones are the purer the more equable the excitement, and the less rapidly they change in intensity. We suppose, further, that noises are produced when the same nerves are excited only for a very short time, or when the excitements pass in irregular and rapid succession from one group of nerves to another, or when they affect so many nerve-fibres simultaneously or in rapid succession, and in such alternation that it is not possible to hear out of the whole impression a tone of definite pitch.

In the view that the same nerves perceive noises if excited for a short time only is included the supposition that there is no fixed limit between tone (or sound) and noise. Between the sound which comes from striking a bell and the noise heard on striking a sheet of pasteboard there is a continuous series in which the vibration dies away so rapidly that no sound is heard. Even then the noise is of different quality according as—chiefly or exclusively—the higher or the lower elements of the organ of hearing are excited. The limit varies so much in different men that one perceives

only a noise, whilst another hears not merely a sound, but can indicate its place in the musical scale. (And those men for whom noise and sound are identical ?)

We have also seen, in a series of explosive noises, that they approximate to quickly fading tones by means of the secondary vibrations of the air, and that, in accordance with the period of these vibrations, they are adapted to excite certain groups of nerve-fibres more strongly than others. For the secondary vibrations are by no means unimportant for the acoustic effect. As far as these groups of nerve-fibres are concerned, they add their effect to that of the primary wave.

These explosive noises possess also the attributes of height and depth, and, as we found the same qualitative difference also in single waves of different times of undulation in which no secondary vibrations have been demonstrated, we concluded that, as theory demands, even the single wave may affect the different fibres of the auditory nerve unequally, according to its period of vibration.

The shrill shriek of a steam-whistle, even when so near that we shrink back from the painful impulse, does not convey to us the joint impression of a deep noise, not even at the beginning, because the single impulse does not agitate the more deeply-toned parts of the auditory organ in so far as to cause a sound-perception.

The case is the same when two short steel cylinders are clashed together close by our ears, whilst the shot of a heavy cannon, miles away, which leaves the high-toned elements of our organ of hearing at perfect rest, occasions the perception of a deep noise. Whether the secondary vibrations (which by their period determine the groups of fibres to be excited) come into play or not, they can strengthen the effect of the former, but they cannot annul the action of the first impulse upon the other fibres if it has already set in.

For the case in which the impulses lie too far apart from each other to occasion a sound, it is unimportant whether they are single or if they die away with a few vibrations determined by the period of the fibre-groups which are excited. The quality of the noise depends only on the wave-lengths as long as the noises are heard singly, and when the impulses approximate and begin to melt into each other the rapid change in the strength of the excitement produces a creaking, and not, in the first place, the sound which ought to be produced in accordance with the period in which the impulses succeed each other.

There remain now, besides the momentary noises, explo-

sions, and blows, the more continuous noises, rustling, murmuring, &c., and we must ask if they necessitate the assumption of special nerves?

In the roaring of the sea and the rustling of the wind among trees there occur very many small explosive noises and blows. We can sometimes hear them separately when we stand quite close to a tree through whose leaves the wind is passing. Along with these small blows there are in rustling also sounds of friction. These, in respect to the sound-waves, seem to bear the same relation as do the small irregular waves of a brook flowing among stones to the waves which arise on the surface of a sheet of water. According to our former observations it does not seem necessary to assume special nerves for the perceptions thus called forth.

Hissing is principally distinguished from rustling by the single noises being of a higher pitch and following more closely upon each other. Breathing and blowing represent the faintest irregular motions of the air perceptible by our hearing, and are also occasioned by friction.

None of these sensations compel us to assume separate nerves for their perception after we have once recognised that we perceive momentary noises, explosions, and blows, by transient excitement of a larger or smaller part of our sound-nerves.

In order to explain the manifold character of these sensations, we have a sufficient basis in the varying wave-lengths and amplitudes of the single impulses; also in their greater or less irregularity, in their quicker or slower succession, in their configuration, and, lastly, in the secondary waves which the first impulse produces by resonance, *e.g.*, on blowing into an empty bottle held before the mouth.

Still, on the other hand, the question arises whether, although we hear noises with our sound-hearing nerves, there may not be also nerves with which we hear noises but not sounds. These would have to be nerves which, on account of the peculiarity of their terminals, are not in connection with any sympathetic tissue, and have not the power of summing up impulses. Nerves exist to which this part might be ascribed, but there are physiological difficulties which, according to Brücke, militate against this supposition.

VII. THE MEXICAN AXOLOTL AND ITS SUSCEPTIBILITY TO TRANSFORMATIONS.

THE prolonged researches of Miss Marie Von Chauvin on the biological relations of the Amphibians have led to most interesting results concerning the transformability of the Mexican axolotl. The observations published by this lady ten years ago proved that under certain conditions, and by certain treatment, it is possible to convert the aquatic axolotl, breathing by means of gills, into the terrestrial *Amblystoma*, which breathes by means of lungs. Individual differences, however, came to light which demanded further enquiry. This has accordingly been carried out, and Miss von Chauvin now lays her conclusions before the public in the "Zeitschrift für Wissenschaftliche Zoologie."

It was soon found that younger specimens could be more easily transformed than older ones, but even among individuals of the same age great differences came to light, depending upon the manner of treatment. Thus axolotls can be more readily converted into Amblystomas if they are kept in water containing little air, and are thus compelled to come more frequently to the surface and to breathe with their lungs. Others, kept in richly aërated water, obtained a sufficiency of air through their gills, and were in consequence less readily converted. It appeared, further, that it is merely requisite to apply any external compulsion towards transformation up to a certain grade of development, and that when this has been reached the animals arrive at the higher form without any further interference.

The point of time when the axolotl has arrived so far in its metamorphosis as to have totally lost the power of living in water does not coincide with the absorption of the gills. On the contrary, the most recent observations prove that the power of the axolotl to live in water may under certain circumstances be retained for a long time in individuals which have become completely adapted to a terrestrial life, and only disappears after the first moulting. Various experiments with axolotls which had passed through this stage proved indubitably that a return to their former life had become impossible; they completed their metamorphosis even though all possible means were taken for its prevention. On the other hand, axolotls which had lived for months in

damp moss, and had breathed with their lungs but had not changed their skins, felt completely at home as soon as they were returned to the water.

This surprising fact induced Miss von Chauvin to institute further experiments on the adaptive power of the axolotl, and in particular to attempt, by suitable treatment, a repeated transformation of these creatures, from the lower to the higher stage, and thence back again to the lower. This interesting experiment has, in fact, been carried out with a successful result.

Without entering upon an account of the means and the precautions used, or a description of the various stages of transformation, we pass at once to the results.

An axolotl lived altogether for three and a half years. The first fifteen months it spent naturally, and without any interference, in the water; its development was then artificially accelerated, and in twelve days it was transformed into a lung-breathing animal, It then lived on the land for fifteen and a half months; it was next, during the lapse of six days, brought back to the water, where it spent three and a half months. In the space of eleven days it was again so modified that it could once more live on the land, where it remained for rather more than six months, up to its death.

The power of adaptation to a change of medium was so distinctly marked in this animal, and was maintained for such a length of time, that Miss von Chauvin instituted a further experiment with axolotls with the object of interrupting at pleasure the metamorphosis of these creatures, and suspending it for years, and subsequently testing their adaptability. For this purpose served five axolotls, each about six and a half months old, in which the development of the lungs was easily so far accelerated that they could live on the land. At this stage the further metamorphosis was suspended by a low temperature and by being placed during the night in water. Nevertheless in one specimen there occurred quite unexpectedly, after the lapse of fourteen months and twenty-two days, the first moulting, followed by the further conversion into the *Amblystoma* form. The four others were kept, however, for three years and two months in the state of suspended metamorphosis. After the expiry of this time the attempt was made to convert two of them back into axolotls, whilst two others were to pass on fully into the *Amblystoma* stage. The result of this experiment was affirmative. The first two specimens reverted to axolotls; of the two latter one died, whilst the other became a perfect *Amblystoma*.

The three experimental subjects whose metamorphosis was arrested on November 8, 1879, were in good health in October, 1884, in spite of the most arbitrary interference with their natural course of development. The *Amblystoma* was indeed smaller than those which had been transformed previously, but it was, nevertheless, very active and greedy, and quite conveyed the impression of a healthy, well-developed animal. In the two axolotls also the arrest of the metamorphosis remained without injurious consequences. They are well developed and feel quite at home in their element. They can be distinguished from normal axolotls merely by a somewhat smaller size, and by a less luxuriant development of the external gills.

The results of these experiments show how exceedingly great is the influence of the surrounding medium upon the organism of animals. Of the most important agents, air, water, and heat, the last possesses indubitably the greatest power over the nature of the animal, and next after it comes the character of the medium in which the animal is compelled to live. The external conditions of life can transform the nature of an animal either by a sudden metamorphosis or by protracted action. But in opposition to all these external agencies there stands a powerful influence seated within the animal and acquired by inheritance which can, indeed, be modified to a certain degree but never entirely suppressed. This circumstance explains both the many individual fluctuations in the results upon perfectly identical treatment and the want of success of so many experiments.

Miss von Chauvin points out that the cases of "neotenism" (persistence of embryonal forms) recently observed among the Urodela find at least a partial explanation in the artificial transformations of the axolotl as here described. For it has been shown that the tendency to continued development can be suppressed by suitable influences, and such influences may make their appearances naturally and involve a persistence of the larval condition.

The importance of this series of investigations on the primitive transit of the vertebrates from the water to the land has been already pointed out. It must not be forgotten that the remarkable tenacity of life of the amphibians is a capital element, both in such natural transit and in the successful result of the experiments described. With insects the case is very different: their metamorphosis has, in certain cases, been suspended, both naturally and experimentally. But all attempts which we have made to induce the reversion of an insect to a larval condition have so far miscarried.

VIII. SILK AND ITS SECRETORS.

WE see a spider spinning its net or a caterpillar lowering itself by a thread from the branch of a tree. But little do we think how many unsolved questions hang suspended on the silken lines. True, we know something about silk; our microscopists teach us that the thread of the silk-worm consists of two solid poreless rods like metallic wires laid side by side and cemented together; whilst the spider's line is composed of many such parallel strands, in some cases over a thousand, though each far more minute than the single line of a caterpillar. Our chemists have analysed this strange secretion and find that it consists of an outer, less compact portion, known in the trade as "gum," whilst within lies the true silk, named fibrione or sericine. Both these parts are composed of oxygen, hydrogen, carbon, and nitrogen, but the gum is less compact than the sericine, and can consequently be removed by a careful application of soap and water.

Last, though assuredly not least, our rogues have made the question of "weighting" or "loading" silk a matter of anxious study and of year-long experimentation, and are now prepared to convert one pound of silk into four or five pounds of something devoid of all the properties which render silk valuable, but which at any rate can be sold as silk.

But all this knowledge takes us but a very little way. Silk, it seems, is a compound of the so-called four organic elements. Its thread has therefore been likened to a thread which we might draw out of a solution of gelatine. But there are some capital differences. All the other nitrogenous portions of the organic system, liquid or solid, if exposed to the air in thin layers, pass very readily into putrefaction and decomposition. Not so silk. We may chop up rags or threads of silk and strew them on the fields or dig them into the soil. They may be exposed to heat and to frost, to sunshine and showers, to dew and wind. But in no reasonable time will they decay and return to the earth. Thence, unlike shoddy, hair, horn-shavings, all very similar to it in composition, save in one point only, silk, however plentiful, is of no value as plant-food. If a farmer were offered five tons of pure silk at ten shillings per ton he would be foolish if he accepted the offer and spread it on his fields.

We said that "in one point only" silk differs chemically from wool, &c., and that one point is very interesting. It contains, like gelatine, no sulphur. But between gelatine and silk the difference is wide. Gelatine dissolves in hot water and softens even in damp air. Silk, as we have just seen, is not acted upon by water. It exists, indeed, in solution in the secretory reservoirs of the spider, the silk-worm, and other creatures. But when once it has been emitted and has dried and hardened, which it quickly does, it shows no disposition to resume a liquid state.

As regards the successive steps of the process by which silk is elaborated from the fluids of certain animals, and, of course, ultimately from their food, there is a sad lack of precise knowledge. One point is certain, that the diet of the silk-spinner has no direct influence upon the composition of the product. We find it yielded, though in varying quantities, by creatures feeding upon every kind of matter which is eatable at all,—upon the juices of living animals, the remains of dead ones, upon wood, leaves, fruits, and the pollen of plants. But all, we believe, eat heartily. The elaboration of a stock of highly nitrogenous matter over and above what is required for growth, for maintenance, and for the reproduction of the species, necessitates an abundance of nutriment.

We come now to the question what animals are silk-secretors, and we may be surprised to find that perhaps half the existing species of animals secrete silk, though in very varying quantities. It may probably be said that all insects which pass through a metamorphosis, that is such as on issuing from the egg are unlike their parents and have to pass through the successive states of a larva and a pupa, will be found to possess glands and reservoirs for the production and storage of this secretion. This would include all the Lepidoptera, probably all the Hymenoptera, a majority of the Diptera and Coleoptera, and some Neuroptera. The Orthoptera, Homoptera, and Heteroptera, which are hatched in forms closely resembling those of the perfect insects, save that the wings are generally absent, do not appear to secrete silk. It will be seen at once that among such a vast multitude of species not all can have been examined for the possession of silk-glands, and the rules we lay down must be regarded as merely tentative.

It is interesting to note that in the genus—so-called—to which the most ancient insects hitherto discovered belong, *i.e.*, the Orthoptera, the locust and grasshopper tribe, the power of secreting silk is not known to exist. Hence we

may not unfairly infer that the secretion of silk is a function which has gradually been evolved since the first appearance of insects upon the earth. We find also that silk-spinning reaches its maximum development among the Lepidoptera, which are generally recognised as the most modern group. We say its maximum because there is not one butterfly or moth known which does not spin to a greater or less extent, whilst the silk-worm and its near allies produce silk in a greater proportion to their own weight than does any other animal.

All the species already noticed, broadly speaking, secrete silk in their larval state only, though indeed it is said that two insects are capable of spinning also when mature. Further observation is here wanting.

But among another Arthropod group, the spiders, the case is very different. Here a metamorphosis does not exist at all, and silk is of course secreted, if at all, by the mature animal.

However, the power of spinning is here less universal than in certain insect genera. Not merely the scorpions and other outlying groups, but many true spiders do not construct webs. Among those which do spin there is a wide range in the quantity of material at the creature's disposal and in the completeness of the work. Yet a certain writer has professed an inability to recognise here the workings of evolution. This is strange, if we reflect that amongst spiders we can trace successive stages, from the single thread used by some roving species to secure themselves against a fall when leaping upon their prey, from the few straggling threads laid upon the very surface of a wall or a tree-trunk, in which a creeping insect may entangle its feet, the shelf-like horizontal webs of the Tegenaria, which are soon clogged and rendered conspicuous by dust settling upon them, to the geometrical net of the Epeira extended in a vertical position between two trees, and which, in some lights, approaching insects seem unable to see. Surely, we repeat, we have here a series of phenomena not opposed to, but in harmony with, the main principles of evolution.

An interesting fact is that the silk-organs in the spider and in the insect, though subserving the same purpose, cannot be pronounced as respectively homologous. The caterpillar and all other spinning insects give out their thread from two organs situate on the upper lip, while the spinnerets of the spider are placed at the posterior extremity of the body and are consequently no modification of the parts of the mouth.

But silk-spinning extends into the world of waters. The Pinna and several mussels have a silk-gland and attach their shells to stones, &c., by a number of threads. It is needless to say that in this case the secreting and spinning apparatus is essentially different from that which we meet with in insects and spiders.

But having traced the production and use of silk so far there comes, perhaps, the hardest question of all: Why does it extend no further? Why do we find no vertebrate animals capable of spinning? It would be too hazardous to say that such a power, if it existed, would be of no use to such animals. If we run over in our minds the various purposes to which silk is applied by the Arthropods, we must admit that, if certain mammals, birds, or reptiles could spin, it would often give them a certain advantage in the struggle for existence. Suppose that a bird or beast of prey could construct a web between trees or in thickets, would not its prospects of securing a fair supply of booty be very distinctly increased? Suppose that a bird when building its nest had the power of tieing its materials together and mooring them to the branches of trees with silk cords, would not its home be more secure? There is however, a reason to the contrary; a net large and strong enough to support, say, an animal of the size and weight of a fox or of a wild cat and in addition the hares, rabbits, birds, &c., upon which these destroyers prey, would have to be very strong, and would require a very large quantity of silk for its construction. Now, as we have seen, so highly nitrogenous a secretion as silk can only be elaborated at the outlay of an excessive quantity of food. It may well be, therefore, that the process would, figuratively speaking, not prove remunerative. In other words, the additional quantity of food captured by the aid of the net might not compensate for the additional quantity expended in the secretion of the silk.

Or, again, it may be that in the vertebrates the power of secreting varied products, colouring-matters, odours, poisons, and also silk is relatively feebler than among the Arthropods.

IX. ON THE SPONTANEOUSLY REVERSIBLE SPECTRAL RAYS,

AND ON THE

ANALOGY BETWEEN THEIR LAWS OF DISTRIBUTION AND INTENSITY, WITH THOSE OF THE RAYS OF HYDROGEN.

M. A. CORNU, in studying the spectroscopic reactions of metals, has come upon some most interesting results, which he has recently laid before the French Academy of Sciences.

The progress of spectroscopic research, and the wonderful extension of its applications, have led up to one of the most beautiful problems ever presented to the experimentalist and the generaliser. If we have given to us the chemical composition of a vapour raised to incandescence under definite conditions, the question is to foresee the distribution of the spectrum, of the radiations emitted and their relative intensity.

Though the attention of observers has often been called to this problem, its solution is by no means very near at hand. M. Cornu, therefore, proposes, after having briefly sketched the points where previous investigations have broken down, to point out an order of phenomena which seem to open out a novel track.

The starting point for all researches of this kind is the following observation:—When we examine the luminous spectra of incandescent vapours we often distinguish groups of rays which repeat themselves with a periodicity more or less regular. These repetitions are still more striking if we extend the scope of our observations up to the extreme limit of the ultra violet spectrum.

The analogy between these periodic groups and the harmonics of sonorous bodies presents itself at once to the mind. The notion is so much the more captivating because the positions of these groups, if considered either on the scale of wave-lengths, or on that of the numbers of vibrations, seem to coincide with the successive terms of the series of whole numbers. It is no wonder, therefore, that so many spectroscopists have yielded to the temptation and have sought to arrange the rays of one and the same spectrum according to a harmonic series.

What a palace in cloudland would G. H. von Schubert and Heinrich Steffens have built up from these analogies had they been known half a century ago!

Unfortunately the simplicity of these numerical relations always vanishes if we substitute for the contemplation of the spectral images a precise numerical determination of the wave lengths. The path of speculation therefore seems in that direction as if brought to an abrupt close.

This want of success, however, should not discourage observers, though the hope of finding a simple law like that of musical harmonics springs from a preconceived idea which should at once be swept aside. This law of whole numbers applies only to a very special form of sonorous bodies, the type of which is a cylindrical column of very great length as compared with its section. If the form of a vibrating body deviates from this special type the relation between the numbers of the vibrations of the successive notes becomes very complex.

This simple remark is sufficient to show that it would be childish to seek for a general law in a relation as simple as that of the harmonics of musical instruments. This would be to invest the structure of incandescent molecules with a mechanical constitution, which neither chemical nor physical phenomena seem to justify. Less simple laws do not succeed better, and this need not surprise us. The foregoing considerations show that we might expect to find, in order to define the distribution of the spectral rays, very complex functions depending on the chemical nature of the element, on the molecular structure of the vapour, and the conditions of vibratory excitement.

In fact what appears to have smitten with barrenness all efforts made towards the solution of the problem, is that observers have thought it necessary to impose upon themselves *a priori* the function destined to represent the succession of spectral rays. On the contrary, M. Cornu thinks it necessary to reject every preconceived idea as to the mathematical expression of the phenomenon, and to search experimentally if there do not exist special functions,—whether expressible or not by the ordinary analytical symbols,—which are capable of uniting under some common law these series whose regularity and periodicity are evidently not fortuitous.

Considered from this point of view the question assumes a novel phase. The immediate problem consists in searching, if there are not found in all spectra certain groups of rays presenting characters in common, independently of the chemical nature of the incandescent vapour.

With these ideas M. Cornu has for a long time pursued the study of the visible and of the ultra-violet spectra. He has succeeded in detecting a class of spectral rays possessing characters so distinct that they cannot be confounded with any others. These are the *spontaneously reversible* rays which M. Cornu described in the "Comptes Rendus" for 1871 (vol. lxxiii., p. 332), and the existence of which has been demonstrated in the spectra of most of the metallic vapours. The common disposition of their groupings, as will appear below, seems to define one of those experimental forms of function answering to the conditions above announced. The following are the facts:—

The spontaneously reversible rays are well known to physicists, and were observed for the first time by M. Fizeau, in the combustion of sodium. An incandescent vapour of small density and at a low temperature emits a certain radiation which corresponds in spectral observation to a certain brilliant but very narrow ray. If we increase progressively the temperature and the density of the vapour, the ray grows both in intensity and in width and becomes a true luminous band. Soon we see appearing upon this band a dark line in the place of the primitive fine ray, and finally the luminous band broadens in a manner somewhat indefinite, still presenting also the dark line, which also expands in a manner about proportional. Upon the luminous ground produced by the expansion of the band, the brilliant, non-reversible rays vanish completely.

These phenomena, observable in the visible spectrum with the majority of the volatile metals, and with the induction-spark, reach enormous proportions with the electric arc, especially in the ultra-violet region. There may be mentioned in particular the ray $\lambda = 228{\cdot}85$ of cadmium. This ray, extremely weak with the induction spark, expands in the arc so as to invade almost the entire photographic spectrum. As for the dark reversal band which occupies its centre, it comes to occupy more than half the interval between the rays 23 and 24 (Mascart's notation), which disappear almost entirely. We may further mention the ray $\lambda = 209$ of zinc; two of the components of the quadruple ray of magnesium $\lambda = 280$, as also certain fine groups in the spectra of aluminium and thallium.

Various observers, above all Messrs. Liveing and Dewar, have observed a great number of curious reversals in the most various circumstances. The most interesting, perhaps, are those of the hydrogen rays in the visible spectrum, and in the ultra-violet that of the ray $\lambda = 285$ of magnesium,

which is easily obtained on burning the metal in the air. It may be added that the so-called *long rays*, defined somewhat arbitrarily by certain spectroscopists, often enter the class of the spontaneously reversible rays.

It is scarcely needful to point out that the ordinary laws of luminous absorption and of refrigeration suffice to explain all these phenomena. But if theory is able to predict the possibility of the reversal of a certain ray it tells us nothing concerning the property of certain radiations to present, under given conditions, spectral reversions to the exclusion of others.

What are, generally speaking, the conditions of temperature, of pressure, and of density the most favourable to the production of this singular emissive power? This, for the present, is difficult to say. We obtain the phenomena by the incandescence of metallic vapours in a space more or less limited, heated by the electric arc or by direct combustion. But it is evident that these conditions have to be determined with more precision, and that they are imperfectly fulfilled by the means used in the production of emission spectra.

(To be continued).

ANALYSES OF BOOKS.

The Winds: an Essay in Illustration of the Principles of Natural Philosophy. By W. LEIGHTON JORDAN, F.R.G.S. Third Edition. London: D. Bogue.

MR. JORDAN is generally looked upon as a heretic in Science, and as a "vera troublesome person." Like many such persons, he has a habit of looking upon familiar phenomena from a novel point of view, and of calling for reasons which our official scientists are not always ready or willing to produce.

The treatise before us forms the third and fourth chapters of a former work of the author's, entitled "The New Principles of Natural Philosophy." One of its objects, as stated in the Dedication, is to show the connection between the Cartesian vortices and the Newtonian laws of gravitation.

The author, in his first chapter, raises the question why the Trade Winds, sweeping along the Earth's surface towards the Equator, do not take their rise in the Arctic regions, but are confined within the Torrid zone, from which an opposite current —the Anti-Trades—sets out, though with less regularity, and passes through the Temperate zones towards the Poles. This feature has puzzled such authorities as Halley and Maury. Indeed the Anti-Trades are exactly the reverse of what the difference of temperature should produce. The author, to meet this difficulty, maintains that the main agency is the gravitation of the Sun and Moon, and indeed, to a less extent, of all extra-terrestrial bodies in conjunction with the centrifugal force of the Earth's rotation. This rotation causes the winds to blow from an intermediate point, or rather from two intermediate points, one in each hemisphere towards the Poles and towards the Equator. The reason why the Trades are more constant than the Anti-Trades lies in the heating action of the Sun, which assists the former, but constantly tends to reverse the latter. The "lagging" of the Trade Winds, and of the Moon in its orbit, accords with, or is not at variance with, the idea of its being due to "the action of a revolving force acting from some point between the centre and the surface of the Earth, and with a force inversely as the cube of the distance from that point." The action of a similar force about the Sun must, the author holds, be the cause of the onward movement of the planets in their orbits around the Sun.

Astral gravitation, it is suggested, is transmuted into light by

the friction of the rotation of the stars, whilst terrestrial gravitation is transmuted into heat by the friction of the Earth's rotation against those rays of light. "In this case the light emanating from the Sun is caused by the same conflicting action of solar gravitation which carries and guides the Earth in its orbit."

We may here remark that the experiments made for the reciprocal transformation of gravitation, and the so-called physical forces or modes of energy, have so far led to no decisive results.

In Part XIII. it is suggested that if the rotation of the Earth were more rapid, its action would carry everything lying loosely on the Earth first to the Equator and then upwards. If it be true that the Earth's rotation is being gradually retarded, the Earth may have passed through a phase similar to that of Saturn, and may once have had its belts. This chapter is accordingly headed "A Belt of Boulders." But is it in evidence that such belts exist on a parallel to the Equator to a greater extent than in other regions of the Earth's surface?

Mr. Jordan considers that the obliquity of the ecliptic to the Equator depends not on cosmical, but "on local terrestrial causes which determine the plane of rotation, and have frequently changed that plane." We doubt, however, very strongly, whether any such changes would account for the epochs of glaciation and for the genial climate of the earlier Miocene.

We have thus presented a brief, and we trust not unfair, view of the author's main ideas, for the demonstration of which we must refer to the work itself.

An Introduction to Practical Organic Analysis. By G. E. R. Ellis. London: Longmans, Green, and Co.

This is a very small book for so wide a subject as organic analysis. It will, however, be at once seen that the author gives instructions merely for the recognition or detection of certain organic bodies, and not for their quantitative determination. Thus what is perhaps too conventionally known as organic analysis, the determination of the four principal organic elements by combustion, is conspicuous by its absence. Numbers of organic compounds also—we may especially mention the colouring-matters—have not fallen within the author's plan. Still we must admit, with some surprise, that considering the very limited compass of the treatise the author has contrived

to describe the characteristic behaviour of a very great number of substances.

The work is, we are told, primarily intended for the use of medical students, and hence we find a chapter devoted to examination for poisons. It is, of course, theoretically speaking, unchemical to classify bodies according to their behaviour with the living animal or vegetable body; but to the medical practitioner, the pharmacist, and the sanitarian this category is very useful. Among the poisons we find no mention of chromium, which is now used on a vast scale in the arts, and may consequently give rise to accidents.

The methods laid down for the recognition of the various substances may be considered as satisfactory.

The least acceptable feature of the book, in our opinion, is that it is written from the "examinational" point of view at present endemic in England.

Notes from the Physiological Laboratory of the University of Pennsylvania. Edited by N. A. RANDOLPH, M.D., and S. G. DIXON. Philadelphia: J. B. Lippincott and Co.

WE have here the records of original investigations undertaken chiefly from the point of view of the practical physician. These papers discuss in succession the fæces of starch-fed infants; the fæces of persons receiving inunctions of cod-liver oil; the distribution of gluten within the wheat-grain; the effects of the administration of turpeth mineral; a reaction common to peptone and the bile-salts; the behaviour of hydrobromic acid and of potassium iodide in the digestive tract; the digestion of raw and boiled milk; the nutritive value of branny foods; a metastatic heat-regulator; the behaviour of petrolatum in the digestive tract; the cutaneous absorption of nicotine; the dietetic factor in the treatment of angina pectoris; a painless escharotic; and the cutaneous absorption of salicylic acid.

The first of these papers, which we have had the pleasure of receiving and noticing in a separate form some time ago, proves that, contrary to the general opinion, many children under three months of age can digest starchy foods.

The second memoir demonstrates that, in a majority of persons, both infants and adults, cod-liver oil applied persistently to the skin makes its appearance in the excretions.

In the paper on wheat-grain the author questions the assumption that the gluten is found almost—if not quite exclusively—in the fourth layer of the grain, as according to Parkes, imme-

diately below and adhering to the third or inner coat of the true bran, whilst the interior of the grain is made up exclusively of starch-granules.

This belief lies at the root of the so-called "bread-reform" movement, and hence, as Dr. Randolph puts it, "many most intelligent persons habitually rasp their digestive surfaces with branny food." The question is one of importance to the entire wheat-consuming portion of the human race, and the author has therefore done good service to the world by demonstrating the presence of gluten in notable quantity in the interior of the grain.

The fifth essay supplies evidence that Millon's reagent is useless as an isolated test, seeing that it reacts with two such distinct compounds as peptone and the bile-salts.

In the seventh paper Dr. Randolph shows that milk is more easily digestible when raw than after boiling; in the latter state it is less sensitive to rennet, but much more to acids, whether strong or dilute.

In the next memoir—a study of the nutritive value of branny foods—Dr. Randolph, after a prolonged course of experiments and observations, concludes that the carbohydrates of bran are digested by man to a slight degree only; that the nutritive salts of wheat are contained chiefly in the bran, and therefore, when bread is eaten to the exclusion of other foods, the kinds of bread which contain these elements are the more valuable. But when, as is usually the case, bread is used along with other foods which contain the inorganic nutritive elements, a white bread offers, weight for weight, more available food than does one containing bran. By far the major portion of the gluten of wheat exists in the central four-fifths of the grain, entirely independent of the cells of the fourth bran-layer (the so-called gluten-cells). Further, these last-named cells, even when thoroughly cooked, are little—if at all—affected by passage through the digestive tract of the healthy adult. In an ordinary mixed diet the retention of bran in flour is a false economy, as its presence so quickens peristaltic action as to prevent the complete digestion and absorption not only of the proteids contained in the branny food, but of other food-matters ingested at the same time. Still, as in the bran of wheat as ordinarily roughly removed, there is adherent a notable quantity of the true gluten of the endosperm, any process which in the production of wheaten flour should remove merely the three protective cortical layers of the grain would yield a flour at once cheaper and more nutritious than that ordinarily used. These results, it must be admitted, are of great practical moment, and seem to us to cut away the ground from under the feet of the bread-reformers.

The tenth memoir relates to a subject which has only recently arrived at practical importance. Most of our readers will be aware that the mineral fats, or more correctly hydrocarbons, have

found their way into the hands of the Parisian pastry-cooks. That when introduced into the digestive organs they should not be assimilated might have been expected. But Dr. Randolph has demonstrated experimentally that this mineral fat is entirely rejected and is found unchanged in the faæs. Hence the conclusion is simply that these hydrocarbons are, as articles of food, quite useless. The author's experiments further go to prove that oily matters are absorbed, not by mechanical, but by physiological action.

The interest of the remaining sections of this book is purely professional.

Bulletin of the Philosophical Society of Washington. Vol. VII. Containing the Minutes of the Society and of the Mathematical Section for the year 1884. Published by the Co-operation of the Smithsonian Institution, Washington.

A GREAT part of this volume is taken up with the annual address of the President, Mr. James C. Welling, on "The Atomic Philosophy, Physical and Metaphysical." This very interesting and important discourse opens with a historical survey of the rise and progress of atomism. The speaker recognises the Atomic Philosophy of the Greeks as the most elaborate and ingenious which has come down to us from all antiquity. He admits that the Epicurean physics are "as much superior to the Aristotelian and the Stoical physics as the ethics of the Lyceum and the Porch are superior to the ethics of the Sty." Yet he at the same time contends, and with fullest justice, that in the whole system there is not an atom of scientific truth. "The whole speculation is a mirage caused by unequal refractions in the Greek intellect." If atoms exist they must needs be chemical conceptions, and the *rationale* of chemistry had not yet dawned on the horizon of the Greek intellect.

This naturally brings us to the much discussed subject of the incapacity of the Greek mind for Science. Mr. Welling criticises the explanation of the fact given by Dr. W. Whewell, who, as our readers will remember, argues that though the Greeks had in their possession an abundance of facts and were acute observers and critics, their ideas "were not distinct and appropriate to the facts." On this he remarks that it would hardly be possible to frame an explanation more pointless. The Greeks had very distinct ideas and their hypotheses were illusory, precisely because they were so "appropriate" to the surface facts of Greek observation. Quoting Professor Jewett, he tells us that "ancient logic was always mistaking the truth of the form for the truth of the matter." He asserts that: "The conscious incapacity of the Greeks for physical science was so great that

we find the very best class of minds among them absolutely revolting at the very idea of such a science. Socrates, for instance, had no patience with it." Now, it seems to us that Mr. Welling somewhat misapprehends the character of the Socratic Apostasis. That he hated what we now call Science is indisputable. That his influence with that of his followers and imitators has mainly contributed to the exaltation of words and dreams and to the neglect of things which has so greatly impeded the advancement of science, and which is even yet defended by the "poor humanist," is equally true. But was he really one of the best class of Greek minds? Would Greek incapacity in Science have remained a conspicuous stumbling-block but for the ascendancy gained by him and by his school? To both these questions we think we may venture to reply in the negative.

To the remainder of this suggestive discourse we have to return on some future occasion.

At the General Meeting of the Society Mr. T. C. Russell read a paper on the glaciers of the high Sierra of California. He points out that the glaciers of the Sierra Nevada were not connected with a northern ice-sheet, but were of mere local origin and of the same type as the Swiss glaciers of the present day, although of far greater magnitude.

A paper read by Mr. C. V. Riley on "Recent Advances in Economic Entomology," contains some passages on which issue may be well joined. The author pleads for what is commonly, though not quite accurately, known as "applied science." He writes: the best results follow when the pure and the applied go hand in hand—when theory and practice are wedded together. But the most profound investigations are generally incapable of any direct application. Who, *e.g.*, can reduce to practice the results of Darwin, of Wallace, of Lyell, and of many, if not most, of their compeers? He says again, in the same spirit: Erstwhile the naturalist was honoured in proportion as he dealt with the dry bones of his science. Pedantry and taxonomy overshadowed biologic research. We may ask with not unjustifiable surprise, where are the pedantry and taxonomy in Gilbert White, in Waterton, in quaint old Rösel, in Swammerdam, and in Leuwenhoeck?

The fact is that in physics and chemistry discoveries often, and that rapidly, pass into or become intertwined with inventions. But in biology, palæontology, psychology, this is not the case. We find here, save incidentally and collaterally, nothing for which a patent may be taken out and a company got up.

Mr. Burnet discussed the question "why the eyes of (some) animals shine in the dark?" This is not due, as some suppose, to phosphorescence, but to light reflected from the bottom of the eye, which light is diffused on account of the hypermetropic condition prevalent in the lower animals. Human eyes, affected

with hypermetropia, do not yield similar results, because the human pupil is too small, and because the bottom of the human eye has not so strongly reflecting a surface as that of most animals. A distinction was pointed outbetween fluorescent and phosphorescent light,—the former disappears as soon as the incident waves are cut off, while the latter does not.

Mr. G. K. Gilbert examines if the rivers of the northern hemisphere being by the rotation of the earth pressed against their right banks, and those of the southern hemisphere against their left banks, this pressure can appreciably modify the courses of rivers?

At the meeting on May 10, 1884, the question "what is a glacier?" was discussed in the form of a symposium. It was not forgotten that hard and fast lines of demarcation do not prevail in nature, but rather gradations of character.

At a meeting on May 24, Mr. H. H. Bates read a paper on the "Physical Basis of Phenomena." In summing up he declares that "It is time to get back to Baconian ground, and while using for its legitimate purposes the magnificent modern machinery in the field of abstract continuity,—extension, motion, duration,—not attempt to conjure with it as a source of objective revelation, which no mere machinery can be. A scaffold of *n* dimensions is as useless to the geometer as to the architect."

Mr. S. M. Burnett read a paper, entitled:—"Are there Separate Centres for Light-form and Colour Perception?" He answers this question in the negative, maintaining that there is no white-light sensation which can be revolved into its constituent elements of colour sensation, and that the sense of form is an expression of the idea of extension as represented by the dimensions of the arc of the retina impressed.

Mr. Washington Matthews sent in a paper bearing the very curious title of "Natural Naturalists," and bringing to light a very curious fact. The author finds that the aborigines of America acquire and disseminate information about creatures which they do not use. "I have never failed," says he, "to get from an Indian a good and satisfactory name for any species of mammal, bird, or reptile inhabitating his country; and I have found their knowledge of plants equally comprehensive. The Indians are in this respect, as a class, "incomparably superior to the average white man, or to the white man who has not made geology and botany a subject of study."

This evidence shows how much our powers of observation have been stunted by the exclusively, or at least mainly literary, character of our educational systems. From childhood our attention is fixed upon words, written or spoken, and except among specialists inobservance has followed.

Papers and Proceedings of the Royal Society of Tasmania for 1884.

We find here an account of the Ceylonese serpent eagle, *Spilornis spilogaster*, with a notice of the distribution of the genus *Spilornis*, a typical Indo-Malayan form which possibly crosses Wallace's line.

Judge Dobson made a timely protest against the wanton destruction of the magnificent tree-ferns of Mount Wellington. He proposes that the Mount Wellington reserve should be made a people's park, with appropriate penalties for the removal of trees and ferns.

Col. Legge, in presenting to the Society a specimen of the brown fish owl, *Ketupa Ceylonensis*, remarked that no large owls were found in Australia except a variety of the barn owl known in England. In Tasmania no species of kite is found, though they are common in Victoria, South Australia, and Western Australia.

According to a paper read by Mr. R. M. Johnston, F.L.S., there is a marked rise and fall in the death-rate both in Europe and Australia, the periodicity of which closely corresponds with the maxima and minima of sun-spots and with the movements of Jupiter in his orbit from aphelion to perihelion.

According to a copy of the "Australian Shipping News" laid on the table by Mr. M. C. Brown, a whale captured in Behring's Straits, in June, 1883, was found, when cut up, to have embedded in his blubber a harpoon with the words "Henty L., 1838" branded upon it. The Henty's, an old colonial family, had in 1838 a whaling establishment at Portland Bay, Victoria.

Mr. R. Henry described the damage to submarine cables in the Eastern seas, inflicted by a species of *Teredo*, which bores through the gutta percha.

In a paper on the Tertiary Flora of Australia, communicated by Baron von Ettingshausen, the opinion is enunciated that the whole existing flora of the world can, in its development, be traced to an universal original flora of by-gone geological ages.

Mr. R. M. Johnston, F.L.S., read a paper on the discovery of two ferns new to Tasmania. These are *Hymenophyllum marginatum*, hitherto only known in New South Wales. and *H. malingii*, supposed to be peculiar to New Zealand. Of the 201 species of ferns found in Australia and Tasmania, only 53 are found in the latter island, and only one of them is peculiar.

The Neanderthal Skull on Evolution. In an Address supposed to be delivered A.D. 2085. By the Rev. BOURCHIER WREY SAVILLE, M.A. London: Longmans and Co.

THIS is, in very truth, a singular production. The common arguments against Evolution, as they are accepted and retailed by men who are not naturalists, are here reproduced in the form of an address supposed to be delivered in St. James's Hall, Piccadilly, two centuries hence. What opinions may be held in the year A.D. 2085 and what may be uttered in St. James's Hall, if that noted locality has not in the meantime been improved off the face of the earth by the dynamitards, we cannot venture to predict. But in view of the fact that the evidence in favour of Evolution is becoming stronger from year to year, it is not rash to assume that before two more centuries have passed, the doctrine of mechanical creation will be, as it is fast becoming, a mere matter of history. We shall return to it when we return to pre-Daltonian chemistry and pre-Copernican astronomy.

Mr. Saville speaks of Charles Darwin with great courtesy, though decidedly underrating his intellectual powers and the value and significance of his results. But whilst repeatedly vindicating our great naturalist from the charge of Atheism, he does not hesitate to quote from a Mr. Noel the following passage:—"The positive evidence of Scripture is against Evolution, whilst on the other side we have purely and simply a theory, whose whole strength depends on the foregone conclusion, that miracles must, if possible, be exterminated." This Mr. Noel must have his interesting side!

In the very preface we find (xx.) a glaring mis-statement. The author states that there are upwards of 20,000 species of animals in creation (had he said 200,000 he would have been still below the truth), and all experience shows that in every instance when crossing species has been attempted, in place of there being any improvement, or the transmutation of one species into another, *sterility* has been the invariable result." This is simply not the fact. We know that the domestic cattle and the bison of North America (familiarly called buffaloes) belong, not merely to distinct species, but to distinct genera. Yet they are capable of fruitful intercourse and the progeny is *not* sterile, as it has been conclusively proved on the large scale. Were it not that the hybrids, like the bison, yield less milk than the common cow and are more given to stray, they would have become common farm-animals. (See Mr. J. A. Allen's "History of the American Bison.")

Among birds cases of fruitful hybrids, if not more common, have been more frequently observed. If Mr. Saville had taken the trouble to refer to Mr. Seebohm's interesting work, "Siberia in Asia," he would have learned that inter-breeding between the

hoodie and carrion crow is common. This ornithologist found such hybrid birds mated together and producing eggs and young. In Germany and Austria the male capercailzie is often found mated with the black hen *(T. Tetrix)*, and the pure breed of the latter is, according to Semper, being, in some districts, superseded by the hybrids.

If sterility is the necessary condition of hybrid animals the same law should necessarily extend to plants. Yet here, according to many observers, including the eminent botanist Prof. Meehan, fertility is the rule and sterility the exception. If we return for a moment to animals we might well ask among how many species has crossing been fairly attempted? Nor must we forget that many animals in captivity, even if matched with their own species, either prove barren or, at least, do not produce viable young.

On the very next stage the author reproduces the threadbare objection that "the supporters of the theory have never succeeded in observing a single instance, in all the millions of years invented (!) in its support, of one species of animal life turning into another." Now, *ex hypothesi*, one species turns into another, not rapidly and as in a transformation scene, but in successive generations, each being born being a shade different from its progenitors. Hence to observe such a change is excluded by the very terms of the question. Does Mr. Saville forget Mr. Herbert Spencer's apologue of the ephemeron which had never witnessed the change of a child into a man? Further, the supporters of the theory have not for millions or even thousands of years had the opportunity of observing all species of animals, or even one species, with sufficient minuteness. Nor must we forget that within the historical ages no one has seen one breed of animal pass into another. Nor does the author ask himself or his readers whether anyone has ever seen a new species of animal make its appearance by mechanical creation.

On the faith of Dr. Elam the author rejects Organic Evolution as "an unverified theoretic conception." Now all working naturalists know that the theory is constantly receiving verifications,—that it accounts for and harmonizes facts which previously were a mere desultory mass. In the same preface the author asserts that he has shown by quoting the *ipsissima verba* that all the scientific celebrities of the 19th century have rejected the Evolution theory, either positively or inferentially.

Now, Mr. Saville has forgotten that a scientific celebrity, who is not a special student of biology, can have on such a question not the slightest claim to authority. Hence, with all due respect to their merits in other directions, we must bow Prof. Max Müller, Clerk Maxwell, Phillips, Main, Challis, Stokes, Tait, &c., out of court. We must remember that Prof. Virchow, though probably the first histrologist in the world, has not studied biology from that point of view requisite for judging of

the origin of species, and that he has recently protested against being set down as an anti-Darwinist; St. George Mivart, though not a Darwinian, is a decided Evolutionist; that the intellect of Agassiz is, on American authorities, supposed to have been in its decay before his rejection of Darwinism, and that Sir R. Owen gives on the question of Evolution a very uncertain sound.

To find Thomas Carlyle, the essentially unscientific or, rather, anti-scientific word-monger, classed among English scientists may make us blush. As to Frank Buckland, though a good observer of facts, he was nothing more. Among "foreign scientists" hostile to Evolution we find mention of Humboldt. Now, Humboldt detested Cuvier, and as far as we can judge was favourable to Evolution.

But if Mr. Saville thinks that the question is to be decided by authority, why does he make no mention of the long and brilliant series of *savants*, Continental, British, and American, who accept Evolution,—men specially qualified as naturalists? Candour certainly dictates that the reader should hear of these men also. In fact, there is in our day scarcely a zoologist or botanist of mark who is not working on Evolutionist lines. It is greatly to be lamented that the subject has not been left to these men, who if they find the theory coming in collision with facts will not keep silent. The intrusion of novelists, divines, lawyers, newspaper writers, &c., merely "darkens counsel."

Bacillary Phthisis of the Lungs. By GERMAIN SEE. Translated and Edited for English Practitioners by W. H. WEDDELL, M.R.C.S., Eng. London: Kegan Paul, Trench, and Co.

THIS work, as may appear from its very title, is distinctly professional in its objects. The author begins with the history and tradition of his subject. Here, as it seems to us, he is guilty of an act of injustice in omitting all mention of Dr. William Thompson, of Melbourne, who most distinctly anticipated Dr. Koch in tracing phthisis to the action of a microbion. There are no grounds indeed for supposing that Dr. Koch was aware of the researches of his predecessor; he made his discovery subsequently, but independently. It is curious to note that whilst Koch has received for his discovery honours and more tangible rewards, Thompson has incurred no little obloquy; and for showing that the Australian climate is no specific for pulmonary consumption he has undergone something very like boycotting.

The author, after his historical sketch and certain definitions, proceeds to the biological study of micro-organisms, firstly in general, and secondly the biology of pathogenic or specific microphytes. He then treats of microphytic maladies, of the forms and culture of the tubercular bacillus and of tubercle in its forms, evolution, and nature. The various causes and conditions of tubercular disease are next taken into consideration.

Here we find an account of the atmospheric causes producing phthisis. There are some interesting facts given concerning the occurrence of micro-organisms in general. They are described as having their maximum in summer and their minimum in spring, though great and prolonged heat diminishes the numbers. The quantity of organisms in the air increases directly as the atmospheric pressure. Dry weather favours them, and so does ozone in small quantities. Above the level of 2000 metres none have been found. The experimental respiration of air, charged with pulverised materials from cavernous expectoration, has invariably conveyed the disease.

Among the conditions favourable to phthisis ranks town-life, especially in over-crowded barracks, shops (*e.g.*, those of the monopolist drapers), schools, prisons, &c. Climate is pronounced to have but a secondary influence. Philadelphia, Marseille, Paris, have a greater proportion of deaths from phthisis than has London.

In the clinical portion of the work phthisis is divided into four categories: the latent, the distinct, the masked, and the doubtful or pseudophthisis.

The chapter on hygiene is very important. It is urged that as far as inoculability is concerned, phthisis presents a perfect analogy with small-pox or syphilis. The contagion is effected in two ways: by the digestive tract through the agency of food, and by the respiratory organs. Both the flesh and the milk of suspected animals should be avoided. Phthisis is very common among oxen and cows, less so among pigs and poultry, but it does not attack sheep. Hence, from this point of view, mutton is the safest animal food. The use of meat raw in the centre of the joint is dangerous. A temperature of 140°, 150° F., in no way destroys its virulent properties. There is no safety unless every portion of the meat has been raised to 212° F. The same temperature deprives the milk of infected animals of its dangerous properties.

Atmospheric contagion is effected by air vitiated by phthisical patients, especially by the dust from articles upon which such patients have expectorated. The great cause of phthisis is therefore urban life: the immigration from the open country into the cities. This is an evil which in innumerable ways is degrading the human race, and which, though the physician can merely point out, ought to be attacked from every possible point. Any and everything which tends to promote this evil should be sacrificed without hesitation.

The author states that the blood of a young child is for a certain time refractory to the microbion. He asks can this stage of resistance not be prolonged? Cannot, so to speak, the soil be sterilised as against this parasite? Among the known and available resources he enumerates gymnastic exercises, such especially as develop respiratory power and capacity, hydropathic means, and lastly, food which must be so regulated that there is a predominance of fatty matter, any excess of potash salts being especially to be avoided. He argues, indeed, that the reason why the herbivora are more subject to tuberculosis than the carnivora is the larger proportion of potash salts necessarily present in their systems.

The value of this work to the medical practitioner is indisputable. But to outsiders, if candidly read through, it may also be of service as opening their eyes to the nature and import of the problems which modern medicine is called upon to solve, and as to the methods which it must employ. If this is done, sneers at the germ-theory may, perhaps, become less frequent.

California State Mining Bureau. Fourth Annual Report of the State Mineralogist, for the year ending May 15, 1884. By H. G. Hanks, State Mineralogist. Sacramento: State Printing Office.

The situation of the Mining Bureau leaves something to be desired. Though the library and museum are very valuable, they are in constant danger of fire. The museum is situate over a stable (!) whence emanate "disagreeable ammoniacal and hippuric odours," whilst the specimens are disturbed in their cases "by the jarring made by the hoisting of hay by tackles attached to the under side of the museum floor." It now contains 6000 specimens, and fresh accessories come in faster than they can be arranged.

The chemical apartment is in abeyance. It is remarked that since the discontinuance of laboratory work, and the discharge of the efficient chemist for want of funds, little chemical work has been attempted. A mining bureau without a well arranged laboratory and an efficient chemical staff seems to us quite a mistake.

The report before us is by no means exclusively confined to the mineral resources of the State. The hydrography, forests, scenery, and climate are also noticed. We find it stated that the coniferous forests alone cover some twenty million acres. That the climate is less extreme than in the central and eastern States. Tornadoes are wanting, and the deaths from sunstroke and lightning do not exceed a dozen. The advantages of Cali-

fornia are certainly not under estimated by the writer or his authorities. Thus we find it asserted that "there can be little doubt but the bulk of the wines turned out in this State will, in the course of a few years, be superior to those produced in any other part of the world,"—a bold saying. It is admitted that the attempts at growing coffee, tea, and tobacco have not been successful. The first of the three is injured by frosts, whilst the leaf of the two latter is wanting in flavour. The American cranberry here mentioned is, we hope, an essentially different fruit from that of central and northern Europe, the flavour of which may be admirably imitated by adding to the sourest red currants a decoction of oak-bark. We are sorry to find that the cultivation of chicory is extending. It is to be regretted that whilst the coffee tree and the vine are decimated by parasites the chicory plant and the sugar-beet have comparatively escaped.

Letter of Mr. W. H. Pickering, entitled "An Attempt to Photograph the Corona." Reprinted from "Science," No. 113, April 3rd, 1885.

Copy of Reply addressed to the Editor of "Science." April 22nd, 1885.

In the former of these two letters Mr. Pickering states, that on occasion of a late partial solar eclipse, he attempted to repeat the experiments of Mr. Huggins on photographing the corona. He describes the arrangements adopted and the results obtained, and draws the following conclusions:—"The conclusions I should draw from my experiments are (1), that, though it is very easy to obtain a corona-like image, one may readily be deceived in such matters, and the same effect obtained by our atmosphere, without the aid of the solar-corona, combined with little defects in the gelatine film (this, I think, is conclusively shown by the extension of the pseudo-corona in front of the moon); (2) that chloride plates are more suitable than bromide for obtaining an atmospheric corona, just as Mr. Huggins has claimed that they are more suitable for taking a solar one; hence I think one must not rely too much on the ultra-violet region sensitiveness of the chloride plate for a separation of the two; lastly, though my experiments fail to corroborate Mr. Huggins's results, they do not, of course, show that his corona may not be solar, but merely indicate that under very favourable circumstances I could obtain no trace of it."

He adds that he has before him a print made from a negative by Dr. O. Lohse, in October, 1878, showing effects very similar to those obtained by himself (Mr. Pickering.) He further adds that Dr. Lohse considers that the halo is wholly atmospheric and not coronal.

In reply, Mr. Huggins points out that the false coronal effects which Mr. Pickering obtained are precisely those to be expected from his methods. Mr. Huggins expresses his suprise that Mr. Pickering should have mentioned his name in connection with experiments carried out in complete disregard to the conditions to which he had called attention as essential. He adds that whatever Dr. Lohse may possibly have stated privately his published statement admits of no such construction as that put upon it by Mr. Pickering.

Think! A Reply to Lord Bramwell's Plea for "Drink." By Dawson Burns, D.D. London: National Temperance Publication Depôt and Alliance Offices.

This publication scarcely ranks among the subjects which we are in the habit of discussing. We may fully concede that alcoholic fluids are not necessaries of life. We may further consider it an error and a chemical sin to use any substance containing a notable proportion of organic nitrogen for any purpose whatever in which such organic nitrogen is not essential, and we may thus pass an unfavourable judgment upon the brewer and the distiller. We may even, going beyond "Teetotaller," express our conviction that in a cold damp climate like ours a man whose food is rationally selected will, under ordinary circumstances, need no liquids of any kind. But at the dictum here quoted from the late Duke of Albany, "Drink—the only terrible enemy England has to fear," we can only smile and think that if this young man had had time to observe more he might have become conscious of the existence of not a few other terrible enemies from which, as a nation, we have much to fear.

Nor can we believe, as do many that the legislative abolition of the drink traffic would land us in Utopia. We believe, with Mr. McElroy ("Popular Science Monthly"), that "with very few exceptions, every man who goes to perdition by the Alcohol route would reach that destination by some other highway if the alcohol line were not running." But on this subject we may refer our readers to the article "Vice as an Eliminative Agent," ("Journal of Science," 1883, p. 273), in which the views of Mr. W. Mattieu Williams and Mr. J. McElroy are expounded and discussed.

CORRESPONDENCE.

*** The Editor does not hold himself responsible for statements of facts or opinions expressed in Correspondence, or in Articles bearing the signature of their respective authors.

CHANGES IN THE ELEVATION OF LAND ROUND THE BALTIC.

"The Swedish Academy recently published ("Nature," Dec. 18, 1884) observations made during the last 134 years, whose result is that the Swedish coast has been steadily rising, while that on the southern fringe of that sea has been falling, the zero line passing from the Schleswig-Holstein coast through Bornholm and Zöland. The northern part of Sweden has risen about 7 feet."

In "On some Properties of the Earth" (1880, p. 376) I said—"But this oscillation will be accompanied by local changes in the volume of the envelope of the earth," &c., "and by a local gradual rising and sinking of continents and seas."

"I presume that the greatest local elevation and depression, within the 595 years period, in the neighbourhood of the mean points and lines (*e.g.*, the Finnian North Cape, and on the same meridian near the Antarctic circle), amounts in both senses, in rising and falling, to 26 feet, or to 13 feet each way together to the $1 \div 595$ part of the mean depth of the sea." With a uniform rise this would be 6·5 feet in 149 years.

O. Reichenbach.

THE BALANCE OF NATURE.

Since game preserving has been almost abandoned in Ireland, it is said that birds of prey have wonderfully increased in numbers and boldness. Kites were common and acted as scavengers

in London, temp. Henry VIII., and perhaps the balance of Nature was not greatly affected by firearms till more than two centuries later, when Fielding said Tom Jones had "actually been known to hit a bird on the wing"; but a change was made in the latter part of last century, for my grandmother had difficulty in guarding my great-grandfather's chickens from kites about 110 years ago, but my father (who was born in 1800) never saw a kite in Yorkshire except once. He tells me an instance of the way in which numbers invite enemies: he has a cat which is a great bird-catcher, and, though you would not expect her to catch swallows, their numbers hovering over the little River Leen tempted her to try. She noticed that they turned from the river over the bank at one spot near a willow: there she lay in the grass, and sprang at them as they passed. One afternoon she brought five (martins) to my father's house.

HUGH BROWNE.

VEGETARIANISM.

YOUR occasional contributor, Mr. Mattieu Williams, has, in a contemporary of yours, advanced a novel and powerful argument in favour of Vegetarianism, *i. e.*, that the disuse of animal food would do away with what you well call the "barbarism" of permanent pasture, and would consequently arrest the depopulation of the open country and the deplorable crowding of the poorer classes into the great cities. But I fear he forgets the moorlands, marsh-lands, &c., which as grazing ground contribute their quota to the national larder, but, being quite unfit for plough or spade, would under a vegetarian *régime* have to be disused.

R. T.

[The moors and mountains might possibly be restored to their pristine condition, that of forests, and the marsh-lands could undoubtedly be utilised as osier-beds.—ED. J. S.]

CREMATION *v.* INTERMENT.

I WAS much surprised to find that an eminent medical authority, when referring—in his recent lecture on Cremation—to the abolition of intramural burial, completely ignored the pioneer of this reform, Dr. G. Walker, long known as "Churchyard Walker." That this gentleman, who alone bore the heat and burden of the day, should thus be ignored is only another instance of the working of the "Sic vos non vobis" principle.

SANITARIAN.

ERRATA.

Page 258, line 3, *for* external world *read* exploded world.
" 260, " 9, *for* natural sources *read* natural causes.
" " " 27, *for* digresses *read* digress.
" " " 35, *for* or so *read* and so.

NOTES.

A Vegetable Lubricant.—In the process of watching the expansion of a terminal bud from a twig of the horse chestnut, I noticed a flocculent substance clothing the petioles of the incipient leaves. Wetted and submitted to a magnifying power of 250 diameters, this substance presented the appearance of a convoluted cluster of perfectly round, transparent snakes, the ophidian extremities being, of course, excepted. I was reminded of a similarly constructed lubricating substance which I had seen many years ago in a hymenomycetous fungus, *Calocera viscosa*, and suspected that the function was alike in both cases. The friction arising from the simultaneous forward and lateral thrusts in the bud of a horse chestnut might be injurious if naked surfaces were in contact during the expansion. But what are the organs forming so perfect a lubricant? Are they modified trichomes,—appendages of epidermal cells? If only a secretion, like gum or mucilage, why should the snake-like forms be so definite? The sticky gum outside the bud is not so constituted. —(Read before the Microscopical Society of Liverpool, May, 1885, by Henry H. Higgins).

The poison of scorpions, according to M. Joyeux Laffuie, acts exclusively on the nervous system.

According to Prof. Thomas Meehan and M. Naudin ("Amer. Naturalist") sterility among hybrid plants is decidedly exceptional.

According to M. F. Folie (Académie Royale Belgique) a current of air rises from the point where it originates, and descends about the point where it terminates.

The "Zoophilist" has recently made an outrageous attack upon Prof. Martin, of the John Hopkins University, and has received a richly deserved castigation. According to "Science" the societies which support the "Zoophilist" include not only "ecclesiastical and courtly dignitaries, but, *proh pudor!* scientific worthies."

According to a contemporary scorpions remain motionless if any person blows strongly upon them in a vertical direction. We have observed the same fact in spiders and in various insects.

Says an occasional contributor to the "Journal of Science" "In England, compromise is the cardinal virtue, and consistency the only unpardonable sin."

The characters by which our predecessors sought to mark off one group of organisms from another are fading away. M. Duval ("Journal Anat. Physiol.") shows that birds possess a placenta, though in a rudimentary form.

According to MM. Pouchet and De Guerne the pelagic fauna of the Gulf of Finland closely resembles that of the large European lakes, as it contains in some parts as little as 0·073 per cent of saline matter.

F. Dahl, in the "Zoologisch Anzeiger," holds that spiders are very short-sighted, and judge by touch of the position of an object which falls into the web.

[This view is supported by many facts in the behaviour of the geometrical spiders.]

The flora of Ceylon includes 3000 flowering plants and 250 ferns: 100 genera of flowering plants found in Ceylon are not met with in the peninsula of India.

At Piltown, in Ireland, the inhabitants use as drinking-water the drainage from an over-crowded graveyard.

We regret having to record the death of Mr. F. Field, our highest modern authority on the mineralogy of South America.

The ovum of the *Ornithorhynchus* has, like that of birds and reptiles, a large yolk for the nutriment of the young.

The peculiar wingless and ground-haunting birds of New Zealand will soon disappear, as animals of the weasel tribe are being imported as a remedy for the rabbit plague.

We learn, from the "American Naturalist," that Dr. Woodrow, of the Theological School at Charleston, has been ejected from the chair for the offence of displaying Evolutionist leanings.

Prof. Noah K. Davis ("North American Review"), discussing the moral aspects of vivisection, concludes that "whosoever hinders the physiologist in his duties, by exciting public odium. commits a trespass on him and on society at large in whose interest he is labouring, and so does a multiplied wrong."

According to "Le Perigard" the western coasts of Europe have, since January, been affected every Saturday and Sunday with barometrical depressions and atmospheric disturbances.

M. Gautier contends that the production of alkaloids is a function of every living cell, animal or vegetable.

In the early morning of May 8th a temperature of 24° F. was registered at Aylesbury, and the following morning one of 28°. It is remarkable that these unseasonable frosts were accompanied by a wind hovering from S.W. to due W., but having all the features of the Polar current.

Earl Onslow, in the House of Lords, recently called upon the Government to "discourage" cremation. We are sorry to find that this great reform receives little support from our medical contemporaries.

M. A. Lavocat points out a new genus in the tetratological family of cyclocephalians. In individuals of this group, for which he proposes the name "ophthalmocephalians," the nose and the eyes are entirely wanting, the place of the orbits being occupied in the median plane by a cavity formed by the junction of the temporal fossæ. The vomer, the ethmoid, and the bones of the nose are entirely suppressed.

Syphilis has made its appearance amongst horses and mules in the neighbourhood of Burgos, and it is feared that the pest may extend to cows, &c. We suppose that the "Anti" genus will rejoice at the extension of the vested interest which they cherish.

Prof. C. E. Bussey ("American Naturalist") gives some interesting facts on the migration of weeds and grasses in Iowa and Nebraska. Old settlers say that notable migrations, not directly due to human interference, have been observed within the last twenty to thirty years.

A correspondent of the French Academy of Sciences observes that phylloxerised vines near Malaga, which had been considered as dead, have sent out vigorous shoots since the earthquakes of last winter!

According to "Cosmos" hens, ducks, and geese having a dark plumage lay earlier and more frequently than those of a light colour. This difference holds good only if the birds live in the open air and are freely exposed to the sun.

"Ciel et Terre" tells us that in France, since 1835, 4609 persons have been struck dead by lightning, 1000 have received fatal injuries, 4000 have been seized with momentary and temporary paralysis, and 20,000 have been struck without sustaining any injury.

The "Lancet" remarks, *haud inepte*, that if Mr. Mundella hopes to sweep back the tide of public opinion concerning the ruinous effects of educational over-pressure, he must find better brooms than Lord Aberdare and Sir Lyon Playfair.

Dr. Lydekker ("Geological Magazine") finds that the phalangeal bones in the British Museum referred to *Dromæus*

sivalensis do not belong to a bird at all, but are the lateral toes of the fore foot of an even-toed ungulate akin to the hippopotamus.

M. A. Cornu ("Comptes Rendus") finds that in the spectra of the metals certain series of rays, spontaneously reversible, present approximately the same laws of distribution and of intensity as do the rays of hydrogen.

Mr. T. A. Jamieson ("Geological Magazine") gives reasons for supposing that the Mediterranean was at one time shut off from the Atlantic; that it was divided into two distinct basins by a land junction extending from Italy to Africa; and that most of its islands, notably Sicily and Malta, were in connection with the mainland.

M. P. Reynard has devised a means of studying the phenomena presented by deep-sea animals when submitted to a pressure of 600 atmospheres.

The eels of the lakes in the Forest of Vincennes perform every spring a pilgrimage to the Seine and the Marne, crossing several kilometres of land.

The Committee on Migration, of the American Ornithological Union, has now, according to "Science," observing stations in every State of the Union, except Nebraska.

According to the researches of Joseph Kleiber ("Astronom. Nachrichten"), the density of the inter-planetary medium at the mean distance of the Earth from the Sun lies between the limits 10^{-22} and 10^{-18} (water = 1).

Councillor Helm, of Danzig, has given his collection of 7000 Coleoptera and 3000 insects in amber to the Museum of West Prussia.

M. de Thierry ("Comptes Rendus") has devised an instrument—the hæma spectroscope—for detecting infinitesimal traces of blood in water, urine, &c. Its action is based on the properties of oxy-hæmoglobin, which gives two absorption-bands between the rays D and E, and of reduced hæmoglobin, which has a single band ("Stokes' band") between the two former.

Mr. C. O. Byrne ("Science"), though no Hampdenite, justifies the epithet of "fanatical pantheist" applied by the Zetetics to the great astronomer, and considers that "nothing can be more disappointing than many of the biographies of physicists, who, even in the most favourable instances, are but little great men."

In opposition to MM. Bouchard and Pouchet, M. A. Villiers ("Comptes Rendus") affirms that alkaloids exist only in pathological urine, and not in the normal excretion.

The "Alnwick Herald" asserts that a boy of 10 years of age, after being ill for three months, vomited a full-grown newt, and thereon completely recovered.

[We once incurred much obloquy for our want of faith in the story that a sickly girl had vomited a lively green lizard, *Lacerta agilis*, which was said to have immediately run away into a hedge.]

Mr. E. Bordoe, in a paper on "Browning as a Scientific Poet," said that, in the poem "Easter Day," Browning seemed to have anticipated by five years Herbert Spencer's theory of the evolution of the first rudiments of nerves in *Medusæ* along the lines of least resistance.

Two very prominent official *savants* will doubtless have resigned their appointments before this is in the hands of our readers.

Prof. O. Marsh proves that there has been a general relative increase in the size of the brain in mammals, birds, and reptiles from the Jurassic to the present time.

Some interesting cases of hermaphroditism in the human subject are being discussed in the medical papers.

In a paper on deaf-mutism an eminent religious organ asserts that all deaf-mutes are found to be entirely devoid of the God-idea,—in other words, natural atheists.

H. Traube ("Neues Jahrbuch für Mineralogie") has discovered a large mass of jade in the Zobtengebirge, near Jordansmühl, in Silesia. This observation is of great anthropological importance.

Dr. de Watteville, writing in the "Lancet," gives an account of the successful treatment of two cases of "writers' cramp," by Mr. Wolff, of Upper Berkeley Street. The agencies used were massage and Swedish gymnastics.

Mr. S. H. Scudder ("Science") contends that no "ordinal differentiation" can be traced in palæozoic insects, though all the existing orders were fully developed by the middle of the Mesozoic period.

Dr. Baur, in the same journal, mentions the exceptional occurrence of a rudimentary second cartilaginous phalanx in the third finger of the wing of an embryonic duck.

The mean duration of life in Bengal is 52·88 years as against 40·86 in England. Taking insanity, blindness, deaf-mutism, and leprosy together, the number of persons affected is in Bengal 3·8 per cent, whilst in England, though we have no leprosy, it is 4·49 per cent.

An apparent epidemic among ducks near Castres, in the department of Tarn, proves to be simply poisoning by the leaves of *Ailanthus glandulosa*, which these greedy birds had eaten in spite of "instinct."

The Rev. J. L. Zabriskie ("Journal of the New York Microscopical Society") shows that the ocelli of the hive-bees, notwithstanding their high convexity, must subserve remote vision, since they are hedged in by dense hairs, with openings suited only to command a view of distant objects.

Can it be believed that, so recently as the year 1880, a man of position and education could be ignorant enough to write in a popular periodical—"Man is distinguished from all other creatures by his possession of mind,—his powers of thought, judgment, and memory"!

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